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THE

AMERICAN

32

JOURNAL OF PSYCHOLOGY

EDITED BY

G. STANLEY HALL

PRESIDENT-ELECT OF CLARK UNIVERSITY

VOLUME II

WORCESTER 1889

~~BALTIMORE, 1888~~

(ISSUED QUARTERLY)

~~N. MURRAY~~, Publisher

E.C. Sanford

PRESS OF ISAAC FRIEDENWALD
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PERSONAL EQUATION.

E. C. SANFORD.

The terms "personal equation" and "personal difference" are somewhat loosely used by astronomers to indicate such systematic errors in observation as originate in the observer, in distinction from those that arise from instrumental and atmospheric conditions. But the errors thus grouped together in their place of origin have by no means the same causes. Some are purely anatomical, such as the constant and clear difference which has been found between observers in setting the cross-wires of a microscope on the division mark of a scale,[1] or in bringing a star midway between two parallel wires, the cause of which seems to be asymmetry of the halves of the eye. To the same general class would belong astigmatism and other structural defects of the eye as far as they inter-

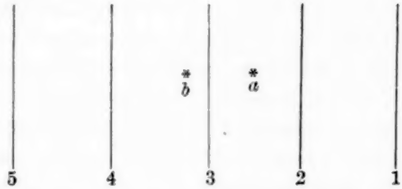
[1] For example, on the limb of a transit circle, or in microscopically comparing standards of length.

fere with observation, and color blindness (if that be an anatomical defect), which has been suggested as explaining the different magnitudes assigned by different observers to the same celestial object. Another set are in part from psychic causes. Such are those that beset observations where judgments of time or space must be made. And others are purely psychic, without physical admixture, like the bias for or against special tenths of a second shown in the recorded observations of some astronomers and recognized more or less consciously by others in themselves. It is, however, a portion of those of the second class that were first noticed, first received the name of personal equation, have since received the most careful investigation, and yet remain the most important. Of the discovery and investigation of these it is the purpose of this paper to give an account.

Every observatory has for one of its chief businesses the fixing of the instant in which heavenly bodies cross its meridian. On this depends the keeping of the true time, and, in connection with the measurement of the distance of these bodies north or south of the equator, the fixing of their positions and motions in the heavens. And in this very process the personal equation is involved. The instrument used for these observations is, in its lowest terms, a telescope mounted on an east and west axis and turning in the plane of the meridian. In the focus of its eye-piece is a set of fine parallel wires or spider-lines from five to twenty-five in number, called a reticle. The middle one of these lies in the meridian. As the image of the star moves across the field, the instant of its bisection by each of these wires is taken, and the average of the times, provided the intervals between the wires are

equal, gives the time of the bisection by the central wire with much less liability to accidental error than if that had been used alone.

At the time of the first notice of personal equation, the method of fixing the instant when the star crossed a wire of the reticle was that of Bradley, or, as it is called, the "eye and ear" method. When the star is about to make its transit, the observer reads off the time from his clock, and then while he watches the star in the telescope, continues to count the second beats. He fixes firmly in mind (as the moving image approaches the wire) its place at the last beat before it crosses the wire and its place at the first beat after, and from the distances of these two points from the wire, estimates by eye the time of the crossing in tenths of a second. A glance at the figure will make the *modus operandi* clear.



The star in most telescopes appears to move from right to left. If we suppose it to be at *a* when the eighth second is counted, and at *b* when the ninth is counted, the time of crossing the third wire will be so many hours, so many minutes, 8.7 seconds. The rôle of the mind in observations by this method is the fixing of the exact place of the star at the first beat, the holding of the same in memory, the fixing of the place at the second beat, the comparison of the two, and the expression of their relation in tenths. When instan-

taneous occurrences like heliotrope or powder signals or the occultation or emergences of stars are to be observed, several ways are open, but the most common ones require the estimation of the fractional part of the second directly by ear. A few astronomers also were accustomed to observe transits in the same way, treating the passage of the wire like an occultation. But this was generally regarded as a vicious aberration from the true method. The psychic action here is a comparison of the two very short intervals of time between the event and the preceding and following clock-beats; or, regarding the whole series of beats, the interpolation of the sudden sensation into their recurring series.

The "eye and ear" method remained the accepted one till about 1850, and is even now more or less used, especially for slow-moving stars like the pole-star. About 1850 the chronographic method of observation was introduced. The chronograph consists essentially in an evenly revolving drum, with which a writing apparatus, under control of an electro-magnet, is connected in such a way that as the drum revolves the apparatus moves slowly from one end of it to the other. If it were undisturbed the pen would trace a spiral line upon the paper with which the drum is covered. But a clock is brought into the circuit with the electro-magnet, and at each second-beat sends a current through it; the magnet draws back the pen and puts a jag in the line for every second except the sixtieth, which is omitted to indicate the minute. A key in the hands of the observer enables him to record his observation by a jag in the same line or a parallel one. All that remains to do then is to indicate the time on the clock to which a certain one of the second-jags corresponds,

and there is a permanent record from which the time of the observation can be read off with ease to a small fraction of a second. By this method of recording the process of observation is much simplified. The astronomer now watches till he sees the star bisected by the wire, then taps his key. He has simply to perceive an event and to will a movement of his finger. The part which the mind plays is thus nearly the same in the observation of transits and sudden phenomena. There is, however, here also a variant application of the method little to be commended. Some observers aim to tap the key so that they shall hear the click of it at the instant of the bisection. They thus add an element of judgment to simple perception and the willing of movement; for to accomplish what they intend, the impulse of will must be given before the star is really behind the wire, and the length of time by which the impulse must precede must vary with the apparent rate of the star. For sudden occurrences they are obliged, of course, to observe like other people.

Now, in all the methods of observation which have been mentioned, observers habitually vary both from the true time and from each other. Their variations from the true time are called their *absolute personal equations;* their mutual differences are their *relative personal equations.* It is natural that the latter should have been first discovered.

So much of a preface has seemed necessary to show what personal equation is. In what follows I propose first to give a brief historical account of the discovery and chief general studies on personal equation, then a more detailed presentation of the circumstances which produce variation in its amount, and, lastly, something of the theories which have been put forward in explanation of it.

THE DISCOVERY OF PERSONAL EQUATION.

The first record of a persistent personal difference between the observations of experienced astronomers goes back a little less than a hundred years. About 1795, Maskelyne, the British Astronomer Royal, noticed such a difference between those of himself and his assistant. At the end of the third volume of the Greenwich Observations he writes as follows:

"I think it necessary to mention that my assistant, Mr. David Kinnebrook, who had observed the transits of stars and planets very well in agreement with me all the year 1794, and for a great part of the present year, began from the beginning of August last to set them down half a second of time later than he should do according to my observations; and, in January of the succeeding year, 1796, he increased his error to eight tenths of a second. As he had unfortunately continued a considerable time in this error before I noticed it, and did not seem to me likely ever to get over it and return to a right method of observing, therefore, though with reluctance, as he was a diligent and useful assistant to me in other respects, I parted with him.

"The error was discovered from the daily rate of the clock deduced from a star observed on one of two days by him and on the other by myself, coming out different to what it did from another star observed both days by the same person, either him or myself . . .

"I cannot persuade myself that my late assistant continued in the use of this excellent method (Bradley's) of observing, but rather suppose he fell into some irregular and confused method of his own, as I do not see how he could have otherwise committed such gross errors."

To the unastronomical mind a difference of eight tenths of a second seems small, but its real signification is more apparent when it is multiplied by fifteen, to give seconds of arc.

For the next twenty years this germ of a discovery lay dormant. But in 1816, von Lindenau mentioned the incident in a history of the Observatory of Greenwich in the *Zeitschrift für Astronomie*, and there it fell under the eye of Bessel, the celebrated Königsberg astronomer. Later, the English Board of Longitude sent the latter a copy of Maskelyne's observations, from which he got a more complete knowledge of the facts. The case impressed him. Considering the easy conditions of such observations with good instruments, and that such were regarded as sure to one tenth or at most two tenths of a second, a difference of eight tenths seemed wellnigh incredible. Its continuance, too, in spite of the desire that Kinnebrook must have felt to bring his observations into harmony with those of his superior, went to prove it involuntary, and therefore important alike to astronomy and anthropology. Bessel desired to know whether such a difference could be found between other pairs of astronomers, and in 1819, while on a visit to Encke and von Lindenau at the Observatory of Seeberg near Gotha, he proposed to test the point with them. Each observed the culmination of several stars, but no second clear night during his stay allowed them to complete the comparison, and the question remained unanswered.

In the winter of 1820–1, at Königsberg, he returned to the subject and made comparisons with Dr. Walbeck, by transits observed on the meridian circle of the observatory. They observed ten stars near the equator

on several nights, each observing five a night, and alternating in such a way that those observed by Walbeck on one night were observed by Bessel the next, and *vice versa*. In this way they arrived at determinations of the rate of the clock which should differ by double the amount of the personal difference, and were thus well calculated to show it if any existed.[1] They found that Bessel was always in advance:

	s.
Dec. 16 and 17	1.145
" 17 " 19	0.985
" 19 " 20	1.010
" 20 " 22	1.025
In the mean,	1.041

This result Bessel considers exact within a few hundredths of a second. The difference was striking on the second day and led naturally to redoubled efforts for accuracy. "We ended the observations," says the astronomer, "with the conviction that it would be impossible for either to observe differently, even by only a single tenth of a second."

Later he repeated the experiment with Argelander, using a little different method. In 1821 he observed seven stars in Gemini, each six times, under favorable circumstances, and their mean position for 1820 was calculated. On two evenings in March and April, 1823, Argelander observed the same stars, while Bessel himself determined the clock corrections. The result-

[1] Suppose the clock to be gaining and that Bessel observes earlier than Walbeck. Then for the stars which Bessel observes first and Walbeck second, the clock rate found will be the real gain of the clock plus the difference of the observers. When Walbeck observes first and Bessel second, the rate found will be the gain of the clock minus the difference of the observers. The difference to the two rates of gain found will be double the personal difference. If the clock is losing, the case is similar.

ing right ascensions were in exces. of those previously found by Bessel, and in excess of what they would have been if Argelander had observed the clock stars himself; that is, the stars appeared to Argelander to cross the meridian later than they did to Bessel. The mean difference for the day in March was 1.222 s., for the day in April 1.224, whence $B - A = -1.223$ s.[1]

Bessel, however, was not content here; Walbeck and Argelander were less practiced in transit observations than he, and he thought that possibly the cause of the difference lay in this. He accordingly asked Struve, of Dorpat, to compare observations with him by means of comparisons with Walbeck and Argelander as they passed through his city. In 1821 Walbeck and Struve observed together on four days, with the resulting equation:

$$S - W = -0.242^{s.}$$

whence $$B - S = -0.799$$

In July, 1823, Argelander obtained the following:

$$S - A = -0.202^{s.}$$

whence $$B - S = -1.021$$

The personal difference, therefore, did not originate in difference in practice.

There is, however, a difference of 0.222 s. between the two values for B — S, and, since there is very little uncertainty in the individual determinations, is evidence of change in one or another of the four observers; most probably in B or S, for the comparison of the intermediary with S was made each time soon after that with B. A single direct comparison points the

[1] The statement of the personal difference in this form has led to its being called the "personal *equation*."

same way. In October, 1814, Struve visited Bessel, and the two observed together; Struve observing the transit of one star, Bessel of two. From these by calculation the equation B — S = — 0.044 s. is found, and though it rests on a single transit, is not without weight, for Struve considered the observation successful, and the agreement of the single wires testifies the same. At any rate the error was not one of eight tenths of a second.

This is sufficient to establish the variability of the personal equation; but later comparisons (leaving Bessel's first study for the moment) give further evidence of the same thing. In 1825 the visit at Königsberg of another astronomer, Knorre, who had just compared with Struve, gave opportunity for repeating the determination of B — S. The result was, B — S = — 0.891 s. A direct comparison in 1834 gave B — S = — 0.77 s. Taking all together we have:

	s.	
1814, B—S	= — 0.044,	direct comparison.
1821	= — 0.799,	indirect "
1823	= — 1.021	" "
1825	= — 0.891	" "
1834	= — 0.770	direct "

Bessel's next thought after having established the fact of a personal difference, was to find its cause. To that end he began to vary the conditions. He first substituted the sudden disappearance or reappearance of a star, as in occultations and emergences, for its steady motion across the reticle. Seventy-eight comparisons of this kind gave for Bessel and Argelander, B′ — A′ = — 0.222 s.; another set of twenty-one gave B′ — A′ = — 0.289 s. A comparison of Struve and Argelander on these sudden phenomena developed no

significant personal difference. Observations of this kind are less certain than transit observations, but they seemed to Bessel to indicate that the trouble lay in combining the steady advance of the star with the sudden beat of the clock, and his next experiment was therefore with a variation in the clock. On two nights he observed a chosen series of stars with a clock beating half seconds, with the following result (indicating by B″ his observations with the half-second clock):

$$\begin{array}{ll} & \text{s.} \\ \text{On the first night } B - B'' = & -0.520 \\ \text{" " second " } B - B'' = & -0.467 \end{array}$$

That is, he observed transits later, on the average, by about half a second in this way than with the whole-second clock. Argelander's observations on the half-second clock compared with those of Bessel made in the ordinary way showed no particular change: $B - A'' = -1.246$ s., or, in another series, $B - A'' = -1.208$ s.

Observations with a half-second clock at Dorpat gave $S'' - A'' = -0.227$; from all of which it appears that Bessel alone had his personal equation changed by the alteration of the rapidity of the beat. One other point the Königsberg astronomer investigated, namely, the effect of the apparent rate of the star, which varies with its declination, on the personal equation. This is of great importance, for if it be found that the rate has no effect, then, provided the personal equation is constant for the time being, it will affect equally the times of transit of all stars observed by the same observer, and will not change at all their relative times of transit, on which their right ascensions depend. Bessel varied the apparent rate of

motion by the use of different powers in his eye-piece, and concluded that the rate had no influence, at least for differences equal to those from the equator to within 30° of the pole.

In brief, Bessel established these points: the fact of personal equation, its spontaneous variation in considerable periods of time, and its artificial change, for himself at least, with change of the clock beat and from transits to sudden phenomena, and he tried, with negative results, the influence of the rate of motion. How important these discoveries are in relation to present knowledge will appear as the narrative proceeds.

Bessel's theory of the psychical cause of the personal equation which he had discovered will be considered elsewhere. In brief it is that the work of the mind is the comparison or superposing of the unlike impressions on the eye and ear, and that observers differ in the readiness with which they accomplish this; an additional difference coming in when one of them goes over from seeing to hearing and the other from hearing to seeing.

It has more than once been noticed as a fortunate coincidence for the knowledge of this matter, that the discoverer of the personal equation should himself have had so large a one; and such it probably was. But its very size has provoked incredulity. It seems simply impossible that two practiced astronomers should observe the transit of a star differently with a clock beating seconds by almost a beat and a quarter. Encke has contended, and after him Wolf, that Bessel must have differed from other observers in the counting of his seconds; counting, for example, a transit which occurred seven tenths of a second after the

pointer of the clock had passed the fourteenth division of the face, as 13.7 seconds instead of 14.7 with other astronomers; in other words, adding the fractional part to the second completed instead of the second begun.[1] The strongest argument in support of this view is that furnished by Bessel's own experiment with the half-second clock, where the average of the two equations obtained gives $B - B'' = -0.49$ s., showing that he observed later by a half second when he timed the transit by half seconds—exactly what would occur if it was his counting that was at fault. And Wolf cites as further evidence the case of a Parisian observer whose results differed from those of his colleagues by a whole second, until the matter was forcibly brought to his attention by setting him to observe the disappearance of a moving object behind an obstacle while he counted the seconds out loud and some one else marked the instant of phenomenon for him by a blow on the back. And there have been other instances of the same kind.

On the other hand, Peters shows that the personal equation between Bessel and Argelander is not unique, as it should be if the former, as Encke says, had counted his seconds "too early as against all other astronomers," but rather the last term of a series of which lower terms can easily be shown. For example:

			s.
Oct. 7, 1833.	Nehus—Wolfers	=	0.62
" 8,	" "	=	0.84
1837.	Gerling—Nicolai	=	0.78
1854.	Main—Rogerson	=	0.70

[1] Such a possibility is also explained as follows: The audible beat of the second is made when the pointer is still moving from mark to mark on the dial. A difference of a second would be introduced if one observer associated the beat with the dial mark which is left and the other with the one that is approached.

To these may be added, not counting Maskelyne and Kinnebrook,

1843. Goujon—Mauvais = 0.58

and 1859. Sashoo—Jacobs = 0.80

Again, if the size of Bessel's personal equation had been due to his method of counting the seconds, his observations of sudden phenomena should have shown it as well as his transit observations; whereas the former gives $A' - B' = 0.222$ s., and the latter $A - B = 1.223$ s. That Bessel's method of counting was the same in the two cases, Peters testifies from conversation on the subject with Bessel himself and from observations made by Bessel in his presence. The presumption is also natural that the possibility of a difference in counting must have suggested itself to a mind so fertile as Bessel's. Wolf is right in judging the fact a hard one to explain on either hypothesis, but it seems to me rather less hard on the supposition that the astronomer counted his seconds correctly than on the other.[1]

Personal Equation before the Invention of the Chronograph.

In the years following Bessel's discovery there were occasional recognitions of personal equation. That of Wolfers and Nehus determined in 1833 has already been given. At an early date, also, Dr. Robinson, of the Observatory of Armagh, noticed a difference of personal equation as the first or second limb of the sun or moon was observed; but not till about 1838 does it

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[1] The fact that Bessel observed with Encke and von Lindenau in 1819 without finding any personal equation has been cited as evidence that he counted his seconds correctly. But as has been said, no personal equation was found, not because there may not have been one, but because the comparisons that were to show it could not be completed.

seem to have received much consideration in actual practice. In the volume of the Greenwich Observations for that year, Airy, the Astronomer Royal, began to publish the figures for the personal equations of the transit observers under his charge. During the two years previous, differences had been noticed, but were too small to be significant. The figures were not found by special tests, but calculated from the clock errors observed in the routine work of the observatory. In the same year, Gerling, professor in the University of Marburg and director of its observatory, published the result of the measurement of the longitude of Göttingen, Marburg and Mannheim, made in connection with Nicolai, Gauss and others. After the measurements had been completed the observers compared among themselves for personal equation, and made the longitude observations uniform by reducing them all to Gerling's own as a norm. The following are the figures obtained by these comparisons:

For transits:

Gerling—Goldschmidt = 0.195 s. from 26 obs. of stars.
Gerling—Nicolai = 0.783 " 72 "
" " = 0.681 from 190 observations of the transits of a spring pendulum.
Gerling—Hartmann = 0.051 from 180 observations of the transits of a spring pendulum observed with a half-second clock.

For flashes of light:

Gauss—Goldschmidt	= 0.088 s.	from	292	observations.
Gerling—Goldschmidt	= 0.027	"	56	"
Gerling—Nicolai	= 0.157	"	308	"
Gerling—Hartmann	= 0.055	"	267	"

As early as 1842 it occurred to Arago that personal equation might be reduced or abolished by giving the observer but one thing to attend to. On New-year's-day, 1843, he applied his idea in the case of a young astronomer, Goujon, whose personal equation usually reached about half a second. He had Goujon indicate the passage of the star by a quick stroke, while another observer, Bouvard, kept the time and estimated the fraction of a second. The personal difference disappeared. To remove the doubt that the difference might have been due to a slowness of hearing, he caused a third person to give the taps while Goujon and Bouvard took the time together. Again they agreed through the forty trials made. During the same year Arago made further tests with a *chronomètre à pointage* which was so constructed that on the pulling of a trigger the second-hand made a dot on the dial from which the fraction of the second could be read off. The observer had only to pull trigger at the instant of the transit and his record was made. With this instrument, Goujon and Mauvais, who otherwise differed by 0.58 s., observed alike. The limit of accuracy in these comparisons was about one twentieth of a second.

In 1843 and 1844, Otto Struve measured a number of personal equations in connection with the determination of the difference of longitude of Pulkowa and Altona, but the figures are of no particular consequence to the subject in hand.

The Invention and Advantages of the Chronograph.

The first attempt at the simplification of transit observations by reducing them to the indication of the instant of passage, and the first suggestion of the chro-

nographic method, antedates the experiments of Arago just recounted by fourteen years. In 1828, J. G. Repsold, director of the Observatory of Hamburg and celebrated mechanician, proposed an apparatus in which the record was taken by means of a point connected with a key on a strip of paper regularly moved by clock-work. In taking a transit observation, the machine being in motion, the observer was first to record a beat of the clock by a tap of the key just before the star began to cross the reticle, then in the same way the crossing of each spider-line as it occurred, and finally the stroke of the clock following the crossing of the last one. From the distances of these dots the time was to be measured off. It was essential, of course, that the strip should move evenly from the first dot to the last, about three minutes for equatorial stars. But the apparatus first made being without a governor, failed in this particular, and the death of the inventor prevented the perfecting of the instrument.

The chronographic method, as it is now practised, is an American product—so distinctly so that it is frequently called "the American method." Of its origin Professor B. Pierce speaks as follows: "The American method is the unquestionable product of the Coast Survey of the United States, and was the legitimate result of the rigid and profound methods of research which are uniformly adopted in this magnificent work. The first conception was in the mind of the superintendent himself, Professor Bache, and its complete development and ultimate success were owing to the united action of Professor Bache and his friend and assistant, Mr. Sears C. Walker. The details of the instrumental invention and execution were intrusted

to Messrs. Saxton, Bond, Mitchel and Locke. Different plans were proposed, but that of Mr. Bond is the one which is at present [1860] adopted in the Coast Survey."

The principle of the chronograph has already been described, and it would be aside from the subject in hand to notice the variations in detail which have been introduced. Suffice it to say here that those in most common use to-day show variations in detail only.[1]

In 1851 the Bond chronograph was exhibited at the meeting of the British Association, and in 1854 the method was introduced at Greenwich. From time to time other observatories have followed and the method is now the accepted one.

The adoption of the chronograph did not do away with personal equation, but it greatly reduced it. Out of thirty-four personal equations determined at Greenwich from 1854 to 1856, only four exceeded 0.1 s., and the highest was 0.17 s.; but in the three years previous, by the old method, out of thirty-three, nineteen exceeded 0.1 s. and eight were over 0.17 s. The difference is due in part to a change of observers, but is nevertheless significant. From an astronomical point of view, however, the increased certainty of the observations is of far greater importance than the lessening of the amount of personal equation. Dunkin found from a comparative study of the observations made on the Greenwich transit instrument in the last year of the "eye and ear" method (1853), with those on the same instrument in 1857, that the probable error of an observation at a single wire by the "eye and ear" method was ± 0.074 s., the probable error of a

[1] Chronographs have been devised which should give the instant of an observation in printed figures, but they have not, I believe, yet reached perfect action.

complete transit ± 0.028 s.; the probable error of an observation at a single wire by the chronographic method was ± 0.051 s., that of a complete transit ± 0.017 s. Approaching the same question again in 1864 from another point of view, he arrives at figures for the probable error of an observation at one wire by the two methods which show the effect of the change on individual observers:

	Henry.	Dunkin.	Ellis.	Various observers, mostly less practised.
Eye and ear,	± 0.112 s.	± 0.062 s.	± 0.069 s.	± 0.089 s.
Chronograph,	± 0.058 s.	± 0.048 s.	± 0.053 s.	± 0.060 s.

Other advantages are credited by Dunkin and others to the chronographic method, but among these the point of special interest in this connection is that the personal equation seems less variable in its amount.

Ways of Determining the Amount of Personal Equation and Devices for Excluding it in Observation.

The reduced personal equation that persisted in spite of the chronograph was still, in the eyes of astronomers, a blemish on the fine accuracy of their science, and from time to time efforts were made for some means, either of determining its amount exactly so that it could be taken into calculation, or of changing the method of observation so as to exclude it. Three ways of determining the amount of personal equation have already been mentioned, that of the Greenwich Observatory, where the custom long has been to get it from the clock corrections found in the routine work of the observatory, and the two ways used by Bessel in comparing with Walbeck and Argelander. Another is the method of divided transits: both observers use the

same instrument and observe the same culmination, one observing the passage over the first wires of the reticle, the other over the last, changing the order in which they observe from star to star so as to exclude possible errors in the corrections for the distances of the wires. This was the method in most common use where personal equation was found from special comparisons. Its chief advantage is that all the instrumental conditions are the same for both observers; its chief disadvantage, provided both are equally accustomed to the particular instrument used, is the hurry of changing places, which might prevent the second observer from observing as he would at his leisure. A special "binocular eye-piece" that was designed to avoid this difficulty was tested at Greenwich in 1852 and 1853. An equilateral prism set in the eye-piece gave two views of the transit from positions 120° apart, thus enabling observers to compare without inconveniencing each other. A method of determining personal equations with transits of the limbs of the sun also allowed simultaneous comparison of a number of observers. The images of the sun and the reticle were projected from the telescope on a table or semi-transparent screen, and the transits were observed as they occurred there. There are still other methods, *e. g.* the observation of the same phenomena with adjacent instruments, or the determination of the longitude of points whose distance is already directly known. But there remains one that deserves attention, namely, that of artificial transits. Its advantage is that the phenomenon to be observed can be produced at any time and as often as necessary. Gerling seems to have been the first to get at the personal equation in this way. When he was comparing him-

self with Goldschmidt and Nicolai, at the suggestion of Gauss, he made use of the transits of an inverted spring pendulum (*Kater'scher Feder-pendel*) in addition to transits of the stars. At an early date Prazmowski used the vibrations of a declination needle for a similar purpose. Such methods are applicable, provided that the personal equation remains the same for the artificial transits as it is for the real, a condition which is probably much better fulfilled in some apparatuses than in others. Against all comparisons by means of real stars, on the other hand, it may be urged that the atmospheric conditions which make a star at one time clear-cut and at another time "woolly," interfere also with the accuracy of the results.

As intermediate between the way of getting rid of personal difference by fixing its amount and allowing for it, and those of excluding it in the observation, two practical devices may be mentioned for avoiding it without knowing its amount. Error is not brought in unless the observations of astronomers between whom such a difference exists are united in computation. This is guarded against by indicating with each observation by whom it was taken. In observations for longitude, however, the combination of the work of two observers is a matter of necessity, and here it is customary for them to exchange stations. Both plans, it will be seen, assume that the personal equation remains practically constant, a thing that seems to be sometimes true and sometimes not.

The devices for excluding personal equation at the moment of observation aim to carry the simplification beyond the point reached by the chronograph. That left the observer free to concentrate his attention on the star; these do away with the motion of the star;

and one even goes so far as to do away with the observer himself. The way in which the first is accomplished is by giving to the whole instrument,[1] or to the reticle,[2] a motion equal to that of the image of the star. This allows the observer, since the motion is under his control, to bisect the image with a line of the reticle as exactly as if both were at rest. When the bisection has been accurately made, the position of the instrument at a certain instant and the time are recorded by the observer or automatically, and from the record the time of the transit of the meridian is calculated. Another means to the same end is instantaneous illumination of the wires.[3] The illuminating flash is made to occur at intervals exactly equal to the time required by the image of the star to move from wire to wire, and its occurrence is recorded, together with the beats of the clock, on the chronograph. The beginning of the series of flashes is under the control of the observer, and is made by him to coincide exactly with a bisection, three or four trials generally being needed; after this the flash repeats itself and its record on the chronograph at each bisection. The rate at which the flashes recur is also adjustable to the declination of the star. The instantaneousness of the flash makes the image to all intents stationary at the instant of bisection.

The difficulty with these methods is the complex apparatus which they require. An instrument of this kind, to be of any service, must be adjustable through a considerable range to the apparent rate of the stars.

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[1] Liais.

[2] Radier and C. Braun. Suggestions for something of the same nature were made by Wheatstone, and, I believe, by A. S. Herschel.

[3] Langley.

The proposal to exclude the observer himself comes from M. Faye. He suggested the substitution of a sensitive plate for the eye of the observer, and the instantaneous photographing of the wires and the image, the instant of the exposure of the plate being recorded electrically. This answers best for transits of the sun where there is plenty of light, but is not impossible for stars. Professor Langley thinks "it is perhaps not too much to say that it will probably be the method of the future."

Investigations of the Absolute Personal Equation.

So far the relative personal equation alone has been spoken of. As long as this alone had been measured, astronomers could be told that though they knew how much they differed among themselves, not one of them knew how much he differed from the truth. They were therefore naturally curious to know what their differences from this were—in other words, what their absolute personal equations were. And the question had besides interesting ramifications into physiology, psychology, and anthropology. Artificial transits and electrical appliances for recording already in use gave the means required for these measurements, and they were soon begun. In 1854, Prazmowski suggested an apparatus for this purpose. It was to consist of a disk carrying a luminous point for a star, and closing an electric circuit the instant the image of the star was bisected by a line of the telescope through which the transit was observed. The second, the instant of observation, and the true time of the transit were to be recorded by electrical means on a moving strip of paper. By varying the distance of the telescope and

the rate of the disk, the conditions of actual observation as regards power of the instrument, rate of the star, etc., could be paralleled. The apparatus could be used for observations by "eye and ear," by taking the seconds from the click of the electro-magnet that recorded the seconds on the strip.

In 1856, Professor Mitchel announced an apparatus for the measurement of absolute personal equation. Two years later he communicated to the English Astronomer Royal the result of a series of experiments on the subject. He used ten artificial stars attached to a revolving disk, recording their real transits electrically, while the observer did the same in a similar way for their observed transits. These records corrected for the errors of the apparatus give the absolute personal equation, or, as Mitchel calls it, the "absolute personality of the eye." The "personality of the eye" he measured both for transits and for the perception of a white stripe on a dark ground; the "personality of the ear" and "personality of touch" likewise by stimuli suited to those organs. He and his assistant, Twitchell, made daily observations for sixty or seventy days, and about thirty persons besides themselves were tested. The following figures are the means of two hundred and fifty-five observations each, the eye stimulus being the white stripe and the ear stimulus a quick tap:

		Mean.	Minimum.	Maximum.
		s.	s.	s.
M.	Eye,	0.161	0.139	0.191
	Ear,	0.164	0.143	0.193
T.	Eye,	0.144	0.118	0.184
	Ear,	0.153	0.129	0.201

Special tests were made to find whether the eye and ear were constant in their "personality" for short

periods of time. Mitchel and his assistant on several days took sets of ten observations each in alternate minutes, and found the eye personality liable to variations of as much as 0.020 s. between the sets of ten. Touch gave results similar to those for the eye, and experiments were not continued.

In taking the artificial transits, Mitchel found that he himself, his assistant, and all the persons tested anticipated the true time. For himself this anticipation was on several occasions as great as 0.1 s. on a mean of ten, and showed somewhat of a daily variation. This led to the trial of artificial emergences and immergences. The first gave results like the simple observation of the white stripe; the second showed the tendency to anticipation and less steadiness. To put what he had discovered to practical use, he replaced the spider-lines in the reticle of his instrument, except the central one, with occultating bars, and observed by immergences, emergences and transits of the central line, but the effect of the change he was not able at the time to report.

In the same year, 1858, Julius Hartmann, Professor in the Lyceum at Rinteln, also published the description of an apparatus for the same purpose, and the results of a study made by means of it. His apparatus consisted of a horizontal clock controlled by a conical pendulum, the regularity of which was tested by a siren. A wheel carrying a three-inch disk of paper was made so as to shunt in or out of the clock system, and could be set so as to produce at any fixed hundredth of a second a sudden flash through a little hole in the disk, or the transit behind a white thread of a steel bead on the surface of the disk. These were to be observed by the "eye and ear" method, the clock

itself giving the second-beats. The conditions could be varied by changing the distance of the bead from the centre of the disk and by changing the distance of the observer. The maximum error of the machine was not more than 0.03 s. or 0.04 s.

In using his apparatus, Hartmann was accustomed, when once the disk was set at any fraction of a second, to let the phenomenon to be observed recur again and again at periods of eight seconds (for light flashes at first even every second) till it could be observed, as it were, at leisure. The result was often a considerable difference in the answers made at the beginning and end of the process. In this way, in his opinion, the observer quickly got an observation free of surprise.

The interest of these repetition experiments is perhaps other than the experimenter realized. Not only is surprise avoided, as he supposed, but the nature of the psychic process is changed. An observer soon catches the rhythm of such a recurring series, and as each member of the series comes, it finds the mind in a state of active expectation. As experimenters in the psychological field have since shown, the reaction time for an expected stimulus is very much abbreviated—so much so, indeed, that the reaction may even precede the stimulus which it should follow. Perception does not then lag behind sensation; the inner or mental series is pushed forward in expectation and synchronized with the outer actual series of stimuli.

The figures for the personal equation found by these experiments are very small, and the mean error for a single observation, since the observer was sometimes ahead and sometimes behind the true time, frequently, if not always, exceeds the average personal equation found.

In observing transits of the bead when it moved along a scale divided to tenths of a second, Hartmann noticed an interesting illusion. Sometimes when he knew beforehand the exact place where the star should be at the second stroke, he seemed to see it from 0.03 s. to 0.08 s. in advance of its true place. With particular effort to see exactly and extreme attention, the star seemed to stand still an instant at the place where it was when the stroke entered. At other times it seemed to advance steadily and was in motion in its right place at the stroke. This happened most frequently at the end of a series of observations or when the experimenter observed somewhat nonchalantly. He does not venture an explanation, but suggests that the differences may be caused by differences of attention; the star being most regarded in the first and third and the clock-beats in the second. Something similar he thinks possibly happens with the flashes of light, though he was not able with his apparatus to demonstrate it.

The conclusions to which this experimenter comes are:

First, that the absolute personal equation, when it amounts to a tenth of a second or more, is not necessarily grounded in the make-up of the eye or ear or in the mind; and that the "reception time" taken alone rarely rises above a few hundredths of a second. Second, that it finds its cause rather in unequal attention, surprise, defective memory of the series of light and sound impressions, wrong customs of observing, etc.—all of which may perhaps be helped by a knowedge of the error. Third, that it is not constant, but varies from day to day and from series to series, and even with the tenth of the second in which the phe-

nomenon happens to fall. A tendency to this last variation Hartmann thinks he is able to find also in the observations of Gauss and Goldschmidt on heliotrope and powder signals in the longitude determination before mentioned.

In 1863, F. Kaiser, of the Observatory of Leyden, published a method of getting the absolute personal equation quite different in the manner of measuring the time from those generally employed. It depends on the observation of the coincidence of the beats of two clocks beating at slightly different rates. An example will show how the thing is done. Suppose the observing clock beats forty-nine times to fifty beats of the standard clock, that is, beats every 1.02 s. At the occurrence of some phenomenon the pendulum of the observing clock is released, and its beats are counted till they reach a coincidence with those of the standard clock. If thirty-five beats are counted, and at the coincidence the standard clock reads 10 h. 42 m. 50 s., the true time of the phenomenon is gotten as follows: $35 \times 1.02 = 35.70$ s. as the time from the starting of the observing clock to the coincidence. This taken away from the time indicated by the standard clock leaves 10 h. 42 m. 14.30 s. as the true time of the phenomenon. To apply this to measuring the absolute personal equation for artificial transits, it is only necessary to have the pendulum held by an electro-magnet which shall release it on the breaking of the circuit, and to make that correspond with the bisection of the star. In Kaiser's apparatus the artificial transits were managed by placing at one end of a bar of wood a lamp, and before it a screen with a small hole in it; at the other end of the bar a lens which projected a fine image of the hole on another screen of oil paper where

the reticle was represented by a vertical black line. The bar was turned by clock-work, could be varied in speed, and was arranged to break the electric circuit at the instant of the passage of the star. When the absolute personal equation was to be taken, the observer made his estimate of the time of the transit by eye and ear, while an assistant counted the strokes up to a coincidence, and from that, as explained above, the real instant of the transit was found ; the difference of this from the observed time gave the personal equation. The apparatus could be varied to represent occultations and powder-signal flashes, and could be applied to measuring the personal equation in observing by the other method, though for that it was more complicated.

Experiments were made from 1851 to 1859. The following figures are from May of the latter year :

Observer.	Mean personal equation.	Prob. error.	Limits between which the personal equation varied.	
Gussew,	-0.10	± 0.057	$+0.07$	-0.31
Brouwer,	$+0.18$	± 0.095	$+0.33$	-0.21
Kam,	$+0.15$	± 0.083	$+0.29$	-0.18
P. J. Kaiser,	$+0.08$	± 0.088	$+0.29$	-0.11

The accuracy of measurements of the personal equation by this method depends on the accuracy with which a coincidence of the clock-beats can be observed. Later experimenters have shown that such observations are themselves liable to a certain small personal error.

In 1862 he improved upon his former apparatus for artificial stars, and adapted the new one to chronographic recording. In the first months of 1867 the Kaisers (father and son), Kam, and Van Hennekeler

made observations to the number of several thousand with this apparatus, from which they concluded that for them the personal equation was small and, provided the circumstances of observation remained the same, constant; that it might be reduced by practice; that the motion of the star, alternately from right to left and left to right, made no difference, except for Van Hennekeler, who observed a little later when the motion was from right to left than when it was the reverse; and that when they observed motions up and down by the introduction of a prism, all were made later.

Another careful measurement was undertaken in 1862 by the Swiss astronomers Hirsch and Plantamour, in connection with the measuring of the difference in longitude between Geneva and Neufchatel. In May, 1861, they determined their relative personal equation by the observation of nine stars, using the chronographic method, and found P—H = — 0.082 s. In October of the same year, with somewhat unfavorable conditions, they found P—H = — 0.202 s. ± 0.020 s., from twenty-three stars with single values ranging from — 0.008 s. to — 0.413 s. In April of the next year, from the observation of twenty-four stars they found P—H = — 0.130 s. ± 0.008 s., with single values from — 0.068 s. to — 0.220 s.

The astronomers were unsatisfied with these results, and went on to the fixing of their absolute personal equations, using the transits of an artificial star, and taking the time with a Hipp chronoscope, measuring down to the thousandth of a second. Their artificial star, as seen in the telescope, was of the second or third magnitude. The disk (a hole in which made the star) was moved by a pendulum, and so fixed that

through its eastward excursion it kept an electric circuit closed and through its westward left it open. In observing, an assistant let the pendulum fall, and this moved the disk from west to east. As the image of the star crossed the wire, the observer gave a signal to a second assistant, who instantly started the chronoscope. As the star crossed the wire on its return, it automatically broke the electric circuit, and thus threw the pointers of the chronoscope into connection with its driving machinery. As soon as the observer saw the star behind the wire, he pressed his key, thus closing the circuit again and throwing the pointers out of connection. The assistant at the chronograph stopped its works, read off the fraction of a second that had elapsed between the opening and closing of the circuit, and the apparatus was ready for another experiment.

The prime fault of the chronoscope for measurements of this kind is that it fails to record anticipatory estimates, and, unfortunately, Plantamour anticipated. To bring his anticipations into calculation, it was assumed that all the estimates would arrange themselves symmetrically about the mean of the figures that would remain after a number of the latest observations equal to that of the anticipations had been temporarily excluded. The difference between the mean of the figures retained and the mean of the figures excluded is by hypothesis equal to the difference between the first mean and the mean of the anticipations, and they can thus be brought into the general average. Hirsch always observed too late, and so his figures needed no correction. Plantamour's corrected averages are also too late.

The following are the values found :

For Plantamour :

		s. s.			
Nov. 4,	2d series,	0.103 ± 0.013	from	45	observations.
"	3d "	0.128 ± 0.014	"	33	"
"	5th "	0.048 ± 0.009	"	41	"
" 5,	1st "	0.069 ± 0.007	"	54	"
"	4th "	0.037 ± 0.006	"	37	"
	Mean,	0.060 ± 0.016			

For Hirsch :

		s. s.			
Nov. 4,	1st series,	0.247 ± 0.043	from	6	observations.
"	4th "	0.178 ± 0.014	"	19	"
"	6th "	0.140 ± 0.007	"	41	"
" 5,	2d "	0.199 ± 0.009	"	22	"
"	3d "	0.169 ± 0.008	"	23	"
	Mean,	0.168 ± 0.013			

From which P—H = — 0.108 s. ± 0.021 s. Or, taking the same by consecutive series:

Nov. 4,	series 2 and 1,	P—H	= — 0.144
"	" 3 " 4,	"	= — 0.050
"	" 5 " 6,	"	= — 0.092
" 5,	" 1 " 2,	"	= — 0.131
"	" 4 " 3,	"	= — 0.133
		Mean,	— 0.114 ± 0.019

The united results of all their comparisons, taken by days and weighted according to their probable errors, gave P—H = — 0.122 s. ± 0.026 s., or grouped by series, — 0.123 s. ± 0.015 s., and these values were finally taken as certain. The mean value of accidental variations in the same series was found equal to 0.056 s.; the value of changes due to the disposition of the

observer ranged between 0.03 s. and 0.04 s. The changes from day to day seem about equal to those from year to year.

One of the most extended studies of the absolute personal equation was made in 1863 and 1864 by C. Wolf, of the Observatory of Paris. Like previous experimenters, he made use of artificial transits. The light of an oil lamp shining through a hole in a black screen made the star, which, by the interposition of lenses, appeared in the telescope as a very small point of light. The screen was moved evenly to and fro by a motor under control of the observer, and its rate could be changed by changing the driving weight. The reticle of the telescope used in the experiments was of five lines. The blunt end of a spring under the screen made contact with an adjustable strip of copper at every bisection of the star by a line of the reticle; or when the direction of its motion was reversed, broke contact at the same point. The making and breaking of the circuit caused the registry of the transit by an electro-magnet on a moving band of paper, where in like manner the clock-beats were also recorded. In some cases registry by an induction spark was substituted. The total length of the apparatus, as set up in Paris, was 4.25 meters. Forty transits, that is, the crossing of the reticle four times in each direction, constituted a full series. The offices of the different parts of the apparatus were so alternated during these as to exclude the errors of the instrument, and determinations by means of it are certain to 0.01 s.

Either the chronographic or the "eye and ear" method might have been studied, but Wolf considered that his personal equation by the chronographic

method would be too small to afford hope of finding its laws, and therefore he confined his attention to the older method. In the first three months of experimentation his personal equation declined from 0.30 s. to 0.11 s., and afterward, under like circumstances, remained constant at that point, which he regards as an evidence of the value of training on appropriate apparatus. Since the observation of the star with motion both from right to left and *vice versa* was essential to the elimination of instrumental errors, Wolf found it necessary to investigate the influence of the direction of motion on the personal equation. The introduction of a total reflecting prism before the eyepiece each time the star moved in one direction or the other, and the removing it when the motion was reversed, made the star appear to move always in the same direction, and gave the means of studying the effect of direction without altering the action of the instrument. He found from twenty-two series of observations, with the star moving at the equatorial rate and the normal power of the eye-piece, the following means for his absolute error:

Motion from left to right,	0.14
" " right to left,	0.10
Difference,	0.04

That is to say, the distance of the star from the line whenever the star was found to the right of it at the beat of the second seemed relatively too large to him. Tests with dots and lines on paper gave a similar illusion and thus fixed its origin in the structure of the eye.

Wolf studied as well the effect of the degree and manner of the illumination of the field, the brightness

of the star, and the position of the head of the observer, without finding that they influenced the size of the personal correction. The rate of the star made a difference. With variations which changed the time of crossing the five lines of the reticle from thirty-one to eighty-seven seconds, he found that his personal equation decreased with the rate, from 0.14 s. in the first case to 0.09 s. in the second ; the star at the equatorial rate crossed in sixty seconds and his personal equation was then 0.11 s. With increase in the power of the ocular his personal equation diminished, but this he does not regard as evidence of a law, because under the higher power a line of the reticle appears to have a sensible breadth, and the place of the star may not always be estimated from exactly the same point of it. And besides, the diameter of the image of the artificial star is increased, which is not the case with actual stars.

The second part of Wolf's study deals with the cause of personal equation, and as this is to be considered elsewhere, only his general conclusions will be given here. He would make three kinds of personal equation : the first, very rare, consists chiefly in a miscounting of the seconds, as is charged against Bessel ; second, a more common form and less in amount, caused by the difficulty of superposing the sensations, an error of imperfect training ; and third, the true physiological personal equation, much smaller and more constant, which is a function of the sensibility of the eye alone. The "eye and ear" method is necessarily better than the chronographic, because the latter involves the same question of the sensibility of the eye, and besides that, of the time necessary to press the key. If it be true, as Dunkin and Pape have

shown, that the average error of a single observation is less by the chronographic method, it only argues that astronomers need more training.

Since the question with artificial-star measurements of the absolute personal equation is whether or not they really correspond to the conditions of actual observation, it is interesting to notice a suggestion made in 1867 by E. Kayser for the determination of the absolute personal equation from the transits of real stars. If a star is bisected by a wire of the reticle of an equatorial instrument, even if the driving clock have considerable errors, it will be able to preserve the bisection for a short time. It is only necessary, then, after making such a bisection, to stop the driving clock on a beat of the time clock and observe the transits of the star across the other wires, the true times of which can be accurately found from the known distances between them. Since the stopping of the clock on a second beat is only hitting one of a rhythmical series, the error need not be large. If the chronograph method is used, the thing is still simpler ; it is only necessary to make the stopping of the driving clock record itself on the chronograph. This gives a beginning from which the time of the transits, also recorded on the chronograph, can easily be read off and the absolute personal equations determined as before.

Since 1867 there have been a number of studies of personal equation, but most of them have been upon changes produced in its amount by varying conditions of observation. These it is proposed to bring in in their appropriate places (together with others passed by in the present narrative) in the next section, which is to deal with the variations of the personal equation.

MEMORY, HISTORICALLY AND EXPERIMENTALLY CONSIDERED.

W. H. BURNHAM, PH. D.

I.

An Historical Sketch of the Older Conceptions of Memory.

I.—*Early Allusions to Memory.*

Mnemosyne, Hesiod tells us, was the mother of the Muses. Without speculating, as some have done, about the reasons for this myth, it is interesting as showing an appreciation of the fundamental nature of memory, and some sort of crude introspective psychology dating back possibly to prehistoric times. Before the art of writing was in common use, men had to depend more largely than to-day upon their memories. It is not surprising, then, that the ancients put a high estimate upon memory before they began to theorize about its nature. There are allusions to memory in Homer and in the Hebrew Scriptures,[1] and occasionally one of the early Greek philosophers tries to explain some phenomenon of memory, but we find no scientific study of the subject before Aristotle.

The psychology of the Ionian school of philosophers, as far as they had any, was sensationalism. Their views of memory must be conjectured from the fundamental principles of their philosophy.

[1] For references see Carus: Geschichte der Psychologie, pp. 150 and 169.

The doctrine of transmigration, as held by the Pythagoreans, was in some degree an anticipation of Plato's doctrine of reminiscence; and Pythagoras's alleged belief that he remembered things that had happened in a former state of existence, would prove that he deemed memory an essential function of the soul, continuing beyond the limits of the present life.

Diogenes of Appolonia is said to have been puzzled by the phenomenon of forgetting. But, in accordance with the principles of his philosophy, he explained it by supposing that the cause of forgetting was an arrest of the equal distribution of air throughout the body. A corroboration of this explanation he found in the easier breathing that follows the recalling of what was forgotten.

Among the Eliatics, Parmenides is said to have held that not only thought, but recollecting and forgetting, depended upon the way the light or heat and the dark or cold are mixed in the body. If we may trust Theophrastus, every presentation, according to Parmenides, corresponded to a definite mixture relation of these qualities, and with the destruction of that relation the presentation disappeared, *i. e.*, was forgotten.[1]

Heracleitus, one might suppose, would study memory carefully; but, in the fragments of his philosophy that have come down to us, nothing is said upon the subject.

II.—*Plato's Doctrine of Memory.*

In Plato we find a more modern psychology. According to him, the thinking power of the mind, the understanding, is above the mere power of sense-perception. It is this power which compares and considers, notes

[1] Cf. Siebeck: Geschichte der Psychologie, 1er Th., 1e Abth.

similarities and contrasts, unity and plurality, and forms ideas of relation between Being and Non-being, as well as of relations of number and proportion. Among the elements of this higher power, recollection (*ἀνάμνησις*) is of prime importance. It rests upon association by similarity or contrast and by simultaneity.[1]

Plato distinguishes the passive retention of perceptions (*μνήμη*) from active memory (*ἀνάμνησις*),[2] and suggests as a definition of *memory*, "the power which the soul has of recovering, when by itself, some feeling which she experienced when in company with the body." He attempts no explanation of memory; but, in the Theaetetus, puts the following words into the mouth of Socrates:

"I would have you imagine, then, that there exists in the mind of man a block of wax, which is of different sizes in different men; harder, moister, and having more or less of purity in one than another, and in some of an intermediate quality. . . Let us say that this tablet is a gift of Memory, the mother of the Muses, and that when we wish to remember anything which we have seen, or heard, or thought in our own minds, we hold the wax to the perceptions and thoughts, and in that receive the impression of them as from the seal of a ring; and that we remember and know what is imprinted as long as the image lasts; but when the image is effaced or cannot be taken, then we forget and do not know."[3]

Plato carries out the same figure to explain different degrees of memory. When the wax in one's soul is deep, abundant, smooth, and of the right quality, the impressions are lasting. Such minds learn easily,

[1] Phaedo, 73 and 74. [2] Phileb., 34. [3] Jowett's translation.

retain easily, and are not liable to confusion. But, on the other hand, when the wax is very soft, one learns easily, but forgets as easily; if the wax is hard, the reverse is true; again, if the wax is hard or impure, the impressions are indistinct, and still more indistinct are they when jostled together in a little soul.

This illustration must not be taken too seriously, for later on in the same dialogue Socrates calls it a "waxen figment," and substitutes for it the figure of the aviary of all kinds of birds—"some flocking together apart from the rest, others in small groups, others solitary, flying anywhere and everywhere." This receptacle is empty when we are young. The birds are kinds of knowledge. Learning is the process of capturing the birds and of detaining them in this enclosure. In acts of memory we re-catch them and take them out of the aviary.

Plato's views upon memory have a special interest on account of their connection with his metaphysical doctrines. Perception and recollection are the occasion of the mind's turning away from the world of sense to the inner world of innate and universal ideas. These ideas we could never get from sense-perception. That gives us only the immediate and the individual. The ideas are of the essential and the universal. We could not conceive them if we did not already know them. Hence the power to know the universal in the individual proves a previous existence in which we had the intuitions of universal truths, and, accordingly, learning is but recollection.[1] The metaphysical aspects of memory, however, let us avoid as much as possible. They would soon lead far from a psychological study.

[1] For references see Zeller's Plato and the Older Academy, pp. 126, 407; cf. also Siebeck: Geschichte der Psychologie.

But this doctrine of recollection lies at the heart of the Platonic philosophy, and it is necessary to note carefully the distinction between it and ordinary memory. The latter as defined by Plato in the passage quoted above, is the memory or recollection of what has been learned through the body, *i. e.*, through sense-perception. It belongs to the world of appearances and is liable to many errors. The former, on the contrary, is not concerned with things of sense. It is recollection of that higher world where we had an antenatal vision of intelligible realities. Its highest manifestation is the insight of the philosopher who sees the divine goodness, truth, and beauty.[1]

III.—*Aristotle on Memory.*

The difference of psychological method in Plato and Aristotle is seen in their treatment of the subject of memory. What Plato says of memory is incidental to the discussion of such profound matters as the nature of the soul and the theory of knowledge. Aristotle devotes a special tract to the subject of memory. While, according to Plato, memory is one of the higher faculties and partakes of the eternal nature of the soul, with Aristotle it is no longer a function of the eternal Nous, but has its seat in the *passive* reason, is dependent upon a physical process, and perishes with the body.

Aristotle seems to have been the first of ancient philosophers to write a systematic treatise on psychology. But, rather curiously, in this work on psychology there is no special treatment of memory. A special

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[1] For the many passages in which the words *μνήμη*, *μνημονείω*, *μνημονικός*, *υνήμων* occur in Plato, cf. Ast: Lexicon Platonicum, II., pp. 356, 357. For *ἀναμιμνήσκω* and *ἀνάμνησις* cf. the same, vol. I., pp. 151, 152.

tract,[1] however, was devoted to the subject. So far as we know, this was the first scientific study of memory; and, for this reason, as well as for its intrinsic merits, the tract deserves special attention. But before passing to Aristotle's doctrine of memory, it is well to notice briefly his theory of sense-perception.

On occasion of appropriate stimuli, movements take place in the sense-organs. These movements, however, are not sense-perception. In perception the mind must compare and distinguish disparate sensations; it must unite the sensations presented simultaneously by our double sense-organs, as of sight and hearing, and it must be conscious of sensation. This work of comparison, psychic synthesis, and self-conscious perception is performed by a central sense. The physical basis of this central sense is the heart. Through it the mind performs the act of sense-perception. Functions now attributed to nervous substance are referred by Aristotle to the *pneuma* connected with the blood. This is the medium by which the movements arising in the sense-organs are transmitted to the heart, and in this *pneuma* the movements persist after the external stimuli have ceased to act.

Incidentally it is interesting to note that, according to Aristotle's psychology, the brain has very little to do with mental activity; to borrow a phrase from Wallace, it serves simply as "a cooling apparatus to counteract the excessive warmth of the heart."

When the movement occasioned in the sense-organ by an external stimulus is propagated to the heart,[2] a

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[1] *De Memoria et Reminiscentia.* For a list of commentators, see Hamilton's edition of Reid's works, p. 891.

[2] The centre of touch and taste, according to Aristotle, is the heart. Sight, sound, and smell have their centre in the brain, but are indirectly connected with the heart.

perception occurs. Sense-perception, then, is an act of the soul by means of a physiological process. In the words of Aristotle, it is a "movement of the soul through the body."[1] This movement may continue after the stimulus which was the occasion of it has ceased to act. The extreme case is the well known phenomenon of a visual after-image. The images of the imagination are such after-sensations. Imagination is weak sensation, or, in the words of Hobbes, "decaying sense." So, too, dreaming is the result of a movement in our bodily organs, caused either from without or from within; and when the violent movements of the day are stilled, feebler movements that were ineffective during the waking hours may cause our dreams. Again, these persisting movements are the elements of memory. Memory and the imagination alike are dependent upon the residua of sensations. The subjective side of a sensation is an image. Thus the proper objects of memory, as well as of the imagination, are images (*φανταστά*). The image, according to Aristotle, is a condition (*πάθος*) of the central sense. Memory *per se* is of the original image or perception; only in an accidental manner does it relate to matters of thought. In other words, abstract ideas and the like are reproducible only so far as they imply images.

At first one wonders how Aristotle will distinguish those movements which constitute memory from those which are the basis of imagination. He is not entirely satisfactory on this point, but he makes this distinction: the picture of the imagination, or the corresponding movement, does not refer to an external object, and is not located in the past. The memory picture, on

[1] De Somno, 1, 454: *ἡ δὲ λεγομένη αἴσθησις ὡς ἐνέργεια κίνησίς τις διὰ τοῦ σώματος τῆς ψυχῆς ἐστιν.*

the other hand, does refer to an object, and carries with it the consciousness of a time in the past when the perception remembered took place.[1] Memory, then, involves time, and both it and the sense of time are dependent upon the central sense.

In his special tract upon memory, Aristotle in part repeats Plato's views, in part discusses the obvious facts of memory which, having been continually repeated since his time, are now mere platitudes, and in part he tries to explain the phenomena of memory in accordance with his general system of psychology. The essay, however, is of special interest because in it Aristotle sets forth clearly the famous doctrine of the association of ideas. Some of the other points of the essay may be briefly mentioned, and special consideration given to the portion relating to association and recollection.

First, Aristotle takes a good deal of space to show, what would seem to be apparent enough to everybody, that memory is of the past, as perception is of the present, and hope and opinion of the future. He notes that the central sense, or sensorium, must be in a condition suitable to receive and retain the impressions. If the sensorium is too hard, no impression is made. If it is greatly agitated, the new movement is ineffectual —on somewhat the same principle, one may suppose, as we say in modern psychology that a weak stimulus is washed out by a strong one. Hence the very young and the very old have poor memories; for the former are in the movement of growth, the latter in that of decay. The question arises: How is it that in recollection we recognize the memory-image as a picture of the absent object? A scholastic answer is given.

[1] See Wallace's Psychology of Aristotle, Introduction, pp. 93–94.

"An animal painted in a picture, he says, is both an animal and a copy, and while being that one and the same, it is nevertheless two things at once. The animal and the copy are not identical, and we may think of the picture either as animal or as a representation. This also is true of the image within us; and the idea which the mind contemplates is something in itself, although it is also the image of something else."[1]

The second chapter of the treatise on memory is devoted chiefly to recollection and the association of ideas. Aristotle distinguishes carefully the mere persistence and direct reproduction of a presentation (*μνήμη*) from voluntary recollection (*ἀνάμνησις*). The latter is indirect reproduction. It is possible only by the association of ideas. The former is an attribute of animals, while the latter is peculiar to man. Recollection occurs according to the sequence of ideas.[2] What and how necessary the sequence shall be depends upon our past experience. "If the sequence be necessary," Aristotle continues, "then when this movement occurs that one will follow. If it is not necessary, but a matter of habit, the latter movement will generally follow."

Sir Wm. Hamilton understands the word translated *movement* (*κίνησις*) as meaning merely *change in quality*. The word, then, he thinks may be fairly translated into modern nomenclature by his famous term *modification*.

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[1] Quoted from George Henry Lewes's Aristotle, p. 257.

[2] *συμβαίνουσι δ'αἱ αναμνήσεις, ἐπειδὴ πέφυκεν ἡ κίνησις ἥδε γενέσθαι μετὰ τήνδε.* This passage is obscure, but it is generally understood to refer to the sequence of motions or the corresponding ideas, and this interpretation agrees with the context. See Hamilton's edition of Reid, pp. 892, 893, and Themistius's Greek Paraphrase of De Memoria, quoted by Hamilton, pp. 893, 894; also Siebeck, Geschichte der Psychologie, Zweite Abtheilung, p. 77; Grote's Aristotle; Grant's Aristotle, p. 170.

One hesitates to criticise such a profound scholar and such a diligent student of Aristotle as Sir Wm. Hamilton; but in the light of what has been said, it seems much simpler, and more in accordance with the psychology of Aristotle, to understand his doctrine of recollection as follows: The *physiological* movements originally connected with a series of perceptions must occur again in the same order when we recall a true memory picture.[1] Man is so constituted that when one movement and the mental image connected with it occurs, another movement with its appropriate mental image is likely to follow. When we would recall anything, then, we must call up idea after idea until we arrive at one upon which in our experience the one we are in search of has often been sequent. Or, in terms of physiology, movement after movement must occur until we arrive at a movement upon which the movement corresponding to the idea desired has often been sequent.

This sequence or association of ideas is subject to certain laws. The remarkable passage in which Aristotle states these laws is translated by Sir Wm. Hamilton as follows: "When, therefore, we accomplish an act of reminiscence, we pass through a certain series of precursive movements, until we arrive at a movement on which the one we are in quest of is habitually consequent. Hence, too, it is that we hunt through the mental train, excogitating [what we seek] from [its *concomitant in*] THE PRESENT OR SOME OTHER TIME, and from its SIMILAR OR CONTRARY OR COADJACENT. Through this process reminiscence is effected. For the movements [which, and by which, we recollect]

[1] Cf. Siebeck: Geschichte der Psychologie, 1er Th., 2te Abth., p. 77 seq.

are, in these cases, sometimes the SAME, sometimes at the SAME TIME, sometimes PARTS OF THE SAME WHOLE."[1] Wallace, quoting the same passage in the introduction to his "Psychology of Aristotle,"[2] gives the following somewhat different and probably more accurate translation: "When engaged in recollection, we seek to excite some of our previous movements, until we come to that which the movement or impression of which we are in search was wont to follow. And hence we seek to reach this preceding impression by starting in our thought from an object present to us, or something else whether it be similar, contrary, or contiguous to that of which we are in search; recollection taking place in this manner because the movements are in one case identical, in another case coincident, and in the last case partly overlap."[3] Whichever translation we adopt, it seems plain enough that Aristotle maintained that voluntary recollection depends upon association by *similarity*, *contrast* or *contrariety*, and *contiguity*. Very likely he meant to include association by *simultaneity* and *sequence*; but any proof of this should rest upon the words used in the text and the general import of the passage, rather than upon doubtful emendations like some of Hamilton's.[4]

A more important question is whether Aristotle meant to limit the application of these laws to voluntary recollection (*ἀνάμνησις*), or whether he intended to

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[1] The Greek is, *διὸ καὶ τὸ ἐφεξῆς θηρεύομεν νοήσαντες ἀπὸ τοῦ νῦν, ἢ ἄλλου τινὸς καὶ ἀφ' ὁμοίου, ἢ ἐναντίου, ἢ τοῦ σύνεγγυς· διὰ τοῦτο γίνεται ἡ ἀνάμνησις. αἱ γὰρ κινήσεις τούτων τῶν μὲν αἱ αὐταί, τῶν δ' ἅμα, τῶν δὲ μέρος ἔχουσιν.*

[2] p. 95.

[3] See also Grote, Grant, Siebeck, and Zeller, op. cit.

[4] After *ἢ ἄλλου τινὸς* in the passage cited above, Hamilton would supply *χρόνου* or *καιροῦ*.

include spontaneous reproduction (μνήμη) as well. The opinion commonly held by students of Aristotle, from Themistius down, seems to have been that he applied the law of association only to voluntary recollection. Hamilton, however, argues forcibly that Aristotle taught the universality of the law of association. It seems natural enough to suppose that one who saw so clearly that in the voluntary train of thought the sequence conforms to the law of association, would have seen that the same laws apply to the spontaneous activity of the mind. But, while Aristotle states the law of association clearly for the former, he at most merely alludes to the latter, and obscurely enough at that.

Later in the same treatise, Aristotle gives an illustration that may serve to elucidate the principles of association as he understood them. In recollection there are certain starting-points or clues. Milk suggests whiteness, whiteness the clear atmosphere, the atmosphere moisture, that the rainy season. So, too, Themistius, in commenting upon the passage quoted above, uses an illustration somewhat similar: "I see a painted lyre, and moved by this as the prior and leading image, I have the reminiscence of a *real lyre;* this suggests the *musician;* and the musician the *song* I heard him play."[1] Again, Aristotle uses an illustration somewhat as follows: Let A, B, C, D, E, F, G, H represent a series of ideas, one of which we wish to recall. From DE as a starting point, we may be moved forward by E, or backward by D, by the association of ideas. If, then, on the suggestion of DE we do not find what we would recall, we may find it by running over the series E...H; if not, we shall at any rate

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[1] Quoted by Hamilton, Reid's Works, p. 901.

find the desired idea by running backwards from D to A. Not much stress, however, should be put upon this last illustration, for the text is so obscure that many different interpretations have been given by commentators. Perhaps Aristotle meant to illustrate something more profound than the mere linkings of presentations in a series and the process of recalling the mental train; but the illustration of such a simple matter as this was not unimportant in the first scientific study of memory. Such commonplace illustrations, however, would hardly be worth repeating, were it not that many have thought the doctrine of association a modern discovery. We have already seen that Plato refers to this doctrine. We shall soon see that St. Augustine held that without the presence of an associated idea we cannot recall a desired thought.

The place of memory in the Aristotelian psychology in relation to the lower psychic activities, is plain from what has been said. The relation of memory as voluntary recollection to the higher activity of the intellect is indicated by Aristotle when he says that recollection is a syllogistic process. Thus it is that while many animals have the lower kind of memory, man alone has the higher form. "The reason is," says Aristotle, "that reminiscence is, as it were, a kind of syllogism or mental discourse. For he who is reminiscent that he has formerly seen or heard or otherwise perceived anything, virtually performs an act of syllogism."[1] With Aristotle, the higher functions of the soul are based upon the lower. "Without nutrition there is no sense, without sense there is no phantasy, without phantasy there is no cogitation or

[1] Hamilton's translation, Vol. II, p. 909, op. cit.

intelligence."[1] The place of memory among the soul's functions is with the phantasy or imagination mediate between sensation and intelligence.

In connection with Aristotle's doctrine of recollection, one passage in his Psychology is interesting, although its importance has, perhaps, been exaggerated. "Recollection," he says, "starts from the soul, and terminates in the movements or impressions which are stored up in the organs of sense."[2] Siebeck interprets this passage as meaning that the soul has the power by means of the heart to effect a sort of efferent movement towards the sense organs, and thus to arouse anew the persisting residua of former motions. Recollection, then, with Aristotle, as in modern psychology, is an excitation, reproduced in a less degree, of the sense organs; and the same organs are excited and the same movements repeated as in the original sensation.[3] This passage is certainly a remarkable anticipation of Bain's famous doctrine that a reproduced impression "occupies the very same parts, and in the same manner" as the original impression.[4]

In the foregoing sketch of Aristotle's view of memory, the attempt has been made to give only what can fairly be found in Aristotle's text. Much of his tract upon memory is obscure. Commentators have held very conflicting opinions in regard to the importance of what he wrote upon association and recollection. Sir William Hamilton calls him the "founder and the finisher of the theory of association," looks upon the commentators as marvellously stupid in their interpre-

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[1] Grote, op. cit., p. 211.

[2] Wallace: Aristotle's Psychology, p. 41.

[3] Siebeck, loc. cit., pp. 78–79.

[4] Bain: The Senses and the Intellect, p. 338.

tations, and deems it a proof of Aristotle's genius that it took the world 2000 years to become intelligent enough to understand him. In reading Hamilton's erudite discussion, one may be led almost to believe that Aristotle was the first Scottish philosopher. But, while Hamilton's Scottish apperception probably found too much in Aristotle's treatise, and while, on the other hand, Lewes may be right in saying that "here, as in so many other cases, modern knowledge supplies the telescope with its lenses," nevertheless Aristotle's doctrine of association was a valuable contribution to science, and it is manifestly unfair to charge him with ignorance of its importance because he did not spin out as many volumes upon the subject as the English Associationalists have done.[1]

IV.—*Conceptions of Memory among the Stoics and Epicureans, and in Cicero and Quintilian.*

The Stoics took Plato's figure of the wax almost literally. They held that the mind is originally a *tabula rasa*. Sensations are the first writing upon this tablet. The object of sensation makes an impression upon the perceiving subject as the seal impresses the wax. Memory depends upon this impression. This was the view of Zeno. Chrysippus found difficulties in such a crude materialistic theory. How could the mind receive and retain at the same time a number of different and partly incompatible impressions? Accordingly he replaced this view by the theory that the sense impression consists in a qualitative change (*ἀλλοίωσις*)

[1] For passages where the words *μνήμη*, *ἀνάμνησις*, etc., occur in Aristotle, see the index in the Berlin edition of Aristotle's Works, Vol. V. In addition to the works cited, see also Waddington-Kastus; De la psychologie d'Aristote, Chap. XIII.

of the passively receiving organ, the soul.[1] The presentation (φάνтασμα) is a state of the soul. The relation of memory to the general theory of knowledge, with the Stoics, was briefly as follows: The lowest act of the soul is mere perception (αἴσθησις); the next is presentation (φαντασία), which adds conscious observation, its function being to make a first test of the truth of the material furnished by sense. If perception has offered a true picture of the external object, this presenting activity of the mind becomes so intensive that the understanding is brought into action. The understanding or judgment approves or disapproves the presentations. If it approves, then arises the empirical fact, which bears upon it the mark of truth. These facts memory stores up. By combination of the separate facts, empirical concepts are formed, which make up the treasure of memory or experience.[2]

The psychology of Epicurus and the other atomists was a simple kind of mechanical sensationalism. Eidola or images from external objects enter the soul through the sense organs. The mind stores up a great multitude of these eidola. Whenever we call up a picture of memory or the imagination, we turn the attention to one of these images. Thus the mind sees in the same way that the eye does, with this difference, that it perceives much thinner eidola.[3]

Cicero and Quintilian[4] both dwell upon the importance of memory, and both seem to adopt the common

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[1] For references see Siebeck's *Geschichte der Psychologie*, p. 209. See also Ueberweg: History of Philosophy, Vol. I, p. 193.

[2] Cf. Stein: Die Erkenntnistheorie der Stoa; Zeller: Stoics, Epicureans, and Skeptics; and Ueberweg, loc. cit.

[3] Lucretius, IV, 750 seq.

[4] Cicero: De Oratore, II, 86 seq.; also Rhet. ad Herenn., III, 16-24; Quintilian: Instit., XI, 2.

theory of the time, that impressions are stamped on the mind as the signets are marked on wax. They are especially concerned, however, with principles relating to the exercise of memory, and they give instructions for mnemonic aids in oratory. Cicero lays special stress upon order as an aid to memory ; and, as sight is the most acute of the senses, those things are best remembered which are visualized by the imagination. In accordance with the ancient mnemonic systems, he would have these imagined forms localized. The advice of Quintilian in respect to memory is especially sensible. According to him, nothing can take the place of exercise and labor. Next in importance is the division and arrangement of one's subject. He notices also the importance of good health ; and says that for slow minds, an interval of rest after study is a good thing to perfect the memory.

V.—*Plotinus on Memory.*

The Neo-Platonic psychology of memory is represented by Plotinus.[1] He discusses the subject at considerable length and presents a somewhat original doctrine. Memory does not belong to God, nor to the divine immutable intelligence in man which knows by direct intellectual perception. It is a function of the soul, and first appears when the world-soul is individualized in bodies. Memory, however, has no basis in the physical organism, nor does the soul impress the sensations upon the body. The effects of sensations are not like impressions made by a seal, nor are they reactions (*ἀντερείσεις*) or configurations (*τυπώσεις*), but the mode of sense-perception is like that of intellectual activity. In memory, too, the soul is active, not pas-

[1] Cf. Enn. IV, *L.* III, *C.* XXV-XXX, and *L.* VI.

sive. The influence of the body proves nothing against this. The changeable nature of the body may cause us to forget, but it cannot condition positive recollection. The body is the river of Lethe, but memory belongs to the soul. The part of the soul to which memory belongs is the image-forming faculty. This holds sense-impressions as well as thought. Two souls, the higher and the lower, are concerned in memory. When the soul leaves the body, the recollections of the lower soul are soon forgotten, in proportion as the higher soul rises toward the intelligible world.

VI.—*St. Augustine on Memory.*

St. Augustine developed the views of the Neo-Platonists in regard to memory. With him, memory is a faculty of animals, men and angels. God, whose immutable essence is above the sphere of movement and change, does not remember. Everything is seen by him in one indivisible and unchangeable present. Augustine does not agree with Aristotle that some animals are devoid of memory. He attributes memory even to fishes, and relates, in confirmation of this opinion, an incident that he had observed. There was a large fountain filled with fishes. People came daily to see them and often fed them. The fishes remembered what they received, and as soon as any one came to the fountain they crowded together, expecting their accustomed food. But Augustine does not suppose that animals have that higher memory which is purely intellectual, although he probably failed to see how purely mechanical and involuntary their so-called acts of memory are.

Memory, with St. Augustine, as in the psychology

of Plotinus, is a function of the soul, not of the body. But, with Aristotle, he refers it to a seat in the physical organism.[1]

What is memory? It is thinking of what one knows. All the various modifications of the soul cannot all be present to us at once. There is a difference between knowing a thing and thinking of it. The musician, says Augustine, knows music, but he does not think of it when he is talking about geometry.[2] The ideas relating to music are in the mind in a latent state. Augustine anticipates Leibnitz in discussing the unconscious modifications of our ideas; but he speaks especially of their gradual decay, while Leibnitz considers the unconscious growth of them. "Many numbers,"[3] he says, "are gradually effaced from memory, for they remain not an instant unaltered. Indeed, what is not found in memory after a year is somewhat diminished even after one day. But this diminution is imperceptible; yet it is not wrongly inferred, for it does not suddenly all vanish the day before the year is up. Hence we may conclude that from the moment it was engraved in memory it began to slip away."[4]

The doctrine of unconscious mental changes and of unconscious mental states is one of the most remarkable features of Augustine's psychology. With irresistible logic he demonstrates the existence of such states in the following passage from another place: "But what when the memory itself loses anything, as falls

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[1] The seat of memory, with Augustine, however, is in the brain, not in the heart, as with Aristotle.

[2] De Trin., *L.* XIV, *C.* VII. See also Ferraz: Psych. de St. Augustin, 2d ed.

[3] Augustine does not mean to limit what follows to mathematical truths, but, according to his psychology, the same would be true of anything that we are liable to forget.

[4] De Musica, *L.* VI, *C.* IV.

out when we forget and seek that we may recollect? Where in the end do we search but in the memory itself? and there, if one thing be perchance offered instead of another, we reject it, until what we seek meets us; and when it doth, we say, 'This is it,' which we should not unless we recognized it, nor recognize it unless we remembered it. . . . For we do not believe it as something new, but, upon recollection, allow what was named to be right. But were it utterly blotted out of the mind, we should not remember it, even when reminded. For we have not as yet utterly forgotten that which we remember ourselves to have forgotten. What, then, we have utterly forgotten, though lost, we cannot even seek after."[1]

It would not be difficult to find passages in modern psychologies that read almost like translations of this chapter of Augustine's Confessions.

Two kinds of memory—sense-memory and intellectual memory—are distinguished in the Augustinian psychology. The former preserves and reproduces not only the images of visible objects, but also the impressions of sounds, odors, and other objects which strike our senses.[2] The images are not like the *eidola* of Democritus, but are ideal, formed by the mind from its own essence. Intellectual memory contains our knowledge of the sciences, of literature and dialectic, and of the questions relating to these subjects.[3] This memory, unlike the memory of sense, contains not the images of things, but the things themselves. These ideas which the intellectual memory stores up are in a sense innate. They never came to us through the senses. They could never have been taught to us

[1] Conf., *L.* X, *C.* XIX. Pusey's translation.
[2] Conf., *L.* X, *C.* VIII. [3] Conf., *L.* X, *C.* IX seq.

unless we had already had them in our memories. "When I learned them, I gave not credit to another man's mind, but recognized them in mine." Thus the memory contains the idea of truth and of God.

Augustine points out too, what has been repeated by Locke and others until it has become a platitude, that we do not remember objects themselves, but the ideas which we have obtained from them. And with his usual subtlety he shows that much of what is ordinarily attributed to perception is really the work of memory.

A French psychologist who has made a special study of St. Augustine, says: "We see what importance St. Augustine attaches to memory. It is, in his view, the faculty which preserves the ideas relating not only to the body but to the soul, not only to eternal truths but to the eternal Being himself. . . . This memory which is peculiar to man and which animals do not possess, this memory which in a mysterious manner contains in it intelligible realities, is, according to the Bishop of Hippo, one of the three great faculties of man and the origin of the other two. It is from it that intelligence arises, and the will proceeds from the one to the other and unites them. Thus, if it is allowed to compare things human with things divine, we have in us an image of the august Trinity. Memory, in which is the matter of knowledge, and which is as the place of intelligible things, offers some resemblance to the Father; the intellect, which is derived and formed from it, is not without analogy to the Son; and love or will, which unites the memory to the intellect, has a certain resemblance to the Holy Spirit."[1]

The well known conditions of a good memory, such as acuteness of sensation, order, and repetition, Augus-

[1] Ferraz: Psych. de St. Augustin, p. 178; cf. also De Trin., *L.* XV, *C.* XXI, XXII, XXIII, and *L.* XI, *C.* VII and VIII.

tine notices only incidentally. More attention is given to the relation of the will to memory and to the association of ideas.

Whether we remember or not depends upon the will. By an act of will we avert the memory from sense-perceptions; as, for example, when we hear a speaker and do not notice what he says, or read a page and do not know what we have read, or walk with our attention upon something else. In all these cases we perceive, but do not remember our perceptions. So, too, recollection depends upon the will: "As the will applies the sense to the body (*i. e.*, external object), so it applies the memory to the sense, and the eye of the mind of the thinker to the memory."[1]

This power of the will over memory is, however, limited by the association of ideas. In order to recall anything by a voluntary effort, we must remember the general notion of the thing or some associated idea. "For example, if I wish to remember what I supped on yesterday, either I have already remembered that I did sup, or if not yet this, at least I have remembered something about that time itself; if nothing else, at all events I have remembered yesterday, and that part of yesterday in which people usually sup, and what supping is."[2] In another place he says that of a series of ideas the lost part is recovered "by the part whereof we had hold."

Many since Augustine have marveled at the miracle of memory. None have expressed their admiration more eloquently. "Great is this force of memory," he exclaims, "excessive great, O my God! a large and boundless chamber: who ever sounded the bottom

[1] De Trin., *L.* XI, *C.* VIII. Pusey's translation.
[2] De Trin., *L.* XI, *C.* VII. Pusey's translation.

thereof? . . . A wonderful admiration surprises me, amazement seizes me upon this. And men go abroad to admire the heights of mountains, the mighty billows of the sea, the broad tides of rivers, the compass of the ocean and the circuits of the stars, and pass themselves by; nor wonder that when I spake of all these things I did not see them with mine eyes, yet could not have spoken of them unless I then actually saw the mountains, billows, rivers, stars which I had seen, and that ocean which I believe to be, inwardly in my memory, and that, with the vast spaces between, as if I saw them abroad. Yet did not I by seeing draw them into myself, when with mine eyes I beheld them; nor are they themselves with me, but their images only. And I know by what sense of the body each was impressed upon me."

It is an interesting fact that Augustine noticed the possibility of illusions of memory. Certain rare phenomena—the so-called recollections of Pythagoras and others who were said to have remembered objects perceived by the senses in a former state of existence—he explains in a very modern fashion, except that he attributes these beliefs to the agency of evil spirits. "For we must not," he says, "acquiesce in their story who assert that the Samian Pythagoras recollected some things of this kind which he had experienced when he was previously here in another body; and others tell yet of others that they experienced something of the same sort in their minds. But it may be conjectured that these were untrue recollections, such as we commonly experience in sleep, when we fancy we remember as though we had done or seen it, what we never did or saw at all; and that the minds of these persons, even though awake, were affected in this way at the suggestion of malignant and deceitful

spirits, whose care it is to confirm or to sow some false belief concerning the changes of souls, in order to deceive men."[1] If they truly remembered such things, he argues, such phenomena would not be so rare, but many persons would experience the same.

Perhaps the most serious criticism of Augustine's psychology of memory is that he almost entirely neglects the physiological side of the subject. He does not even notice the relation of memory to states of health or disease, and of youth or age. In one place, however, he states that memory has its seat in one of the three ventricles of the brain which is situated between that which is the seat of sensation and that which presides over locomotion, so that our movements may be co-ordinated.[2] Certainly in some passages he seems to make memory contain a kind of innate ideas that may be drawn forth by suggestion.[3] But if here Augustine is unsatisfactory, it must be remembered that he is not writing a psychology, and that he was, as Ferraz calls him, a philosopher of transition. "He combats Plato's doctrine of reminiscence, and prepares the way for the innate ideas of Descartes, without positively enough rejecting the former and without clearly enough admitting the latter."[4]

The criticism has also been made that Augustine seems to waver in his conception of memory, that he sometimes represents it as the source of all our intellectual activity, comparing it among the other faculties to the Father in the Trinity, and that again he seems to limit this faculty to the work of preserving knowledge acquired empirically.

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[1] De Trin., *L.* XII, *C.* XV. Haddon's translation.

[2] De Gen. ad Litt., *L.* VII, *C.* XVIII.

[3] Conf., *L.* X, *C.* X and XI. [4] Ferraz, op. cit., p. 192.

Without lingering upon the views of the other Church fathers,[1] Nemesius may be mentioned as an illustration of the continued Platonic influence. The soul is divided, according to him, into three parts, the perceiving (φανταστικόν), the thinking (διανοητικόν), and the remembering (μνημονευτικόν) faculties. The physiological basis of the third is the *pneuma* in the posterior ventricle of the brain. Besides the two aspects of memory usually mentioned, *i. e.*, retention and recollection, he treats reminiscence in the Platonic sense, *i. e.*, the becoming conscious of innate ideas.[2]

VII.—*Diseases of Memory mentioned by the Ancients.*

The pathological side of memory seems to have been little studied by the ancients. Augustine referred to the possibility of illusions of memory, in the passage already cited. Seneca tells of a certain Sabinus who had so bad a memory that he forgot the name of Ulysses, and again of Achilles, and sometimes of Priam, though he knew them as well as we remember our schoolmates.[3] Some remarkable cases of amnesia were reported by the elder Pliny. "Nothing whatever in man," he says, "is of so frail a nature as the memory; for it is affected by disease, by injuries, and even by fright, being sometimes partially lost, and at other times entirely so. A man who received a blow from a stone forgot the names of the letters only; while on the other hand, another person who fell from a very high roof could not so much as recollect his mother, or his relations and neighbors. Another person, in con-

[1] For an interesting criticism of Plato's doctrine of oblivience, see Tertullian, De An. Ch. XXIV.

[2] Cf. Siebeck, op. cit., 1er Th., 2te Abth., pp. 399, 400.

[3] Epistolae, 27.

sequence of some disease, forgot his own servants even ; and Messala Corvinus, the orator, lost all recollection of his own name." While these cases are good illustrations of certain diseases of memory, they are not reported with sufficient accuracy and detail to render them of much scientific value. Ancient thinkers appear not to have appreciated the importance of studying the pathological conditions of memory.

VIII.—*Conceptions of Memory in the Middle Ages.*

The views of the scholastics need not detain us long. They seem to be generally developments, either of the views of Aristotle or of Plato and St. Augustine. Avicenna and others divided the inner or central sense of the Aristotelian psychology into five inner senses, of which one was memory. We have seen that, according to Aristotle's psychology, in perception the form or image of a thing enters the soul, being the subjective correlative of organic movements occasioned by external stimuli. In the scholastic psychology this view of perception becomes the doctrine of sensible species. The idea of memory held by many[1] was somewhat as follows : The impressions made by objects of sense are preserved by the mind. In recollection and imagination, the inner species corresponding to an external object can be formed by the aid of the physical organism without the actual presence of the object. The view of Albert the Great in regard to the localization of memory is interesting. The sensorium is in the anterior portion of the brain, judgment and the image-forming faculty farther back, and memory

[1] Cf. Siebeck, op. cit., 1er Th., 2te Abth., pp. 418, 433, for the views of Hugo of St. Victor and Avicenna.

and recollection in the posterior portion.[1] The psychology of St. Thomas Aquinas, the famous pupil of Albert, is largely that of Aristotle. He speaks of the memory as the *vis apprehensiva præterita*, and connects it with the sensory side of the mind, through the common or inner sense.

Under the impulse of the Reformation, a new Aristotelianism arose. Its founder was Melancthon. He differs from Aristotle in making memory a function of the intellect, thus vindicating for it the immortality that Aristotle attributed to the active intellect.[2]

Ludovicus Vives, an Aristotelian of the sixteenth century, devoted considerable attention to memory and the association of ideas.[3] He wrote in a practical way about memory, and was, perhaps, the first to mention that mnemonic device which so many have found useful, *i. e.*, the writing of what one would keep in mind. It is well, he says, to write what we would remember; the pen writes upon the heart as well as upon the paper. His doctrine of association is Aristotelian.

IX.—*Conceptions of Memory in Cartesian Philosophy.*

The notion of the correlation between physical and psychic processes was clearly understood by the dualists of the seventeenth century. Chauvin's Thesaurus speaks of a threefold memory—that of the mind exclusively, that of the body, and that of mind and body. Of the last he says: "Memory of the mind and of the body consists in a constant relation between the thinking of the former and the motion of the latter,

[1] Cf. Siebeck, loc. cit., p. 431.
[2] Cf. Ueberweg, Hist. of Phil., Vol. II, pp. 16–19.
[3] Cf. his Opera, passim, Basileae, 1555.

such that when a thought is recalled a movement is renewed; for the one state seems to call forth the other."[1]

Descartes explains the physical processes involved in memory in accordance with his crude physiology, and bridges the chasm between mind and matter with dogmatism. "When the mind wills to recall anything," he says, "this volition causes the pineal gland to incline itself successively this way and that, and impel the animal spirits to various parts of the brain until they come to that part in which are traces left by the object we would remember." The nature of these traces is explained as follows: The pores of the brain, through which the animal spirits have once passed, acquire a tendency to open again in the same way to them as they come again, so that these spirits finding the same pores again enter them more easily than others. "In this way the spirits arouse a special movement in the gland, which represents the same object to the mind, and shows it that the object is the same which it wishes to recall."[2] In the passages quoted it will be observed that the mental process is not put as the result of the physical process, as in some later writers, but rather the action of the mind is emphasized as originating the physical process. Really, however, in the Cartesian philosophy neither process was looked upon as originating the other, but the constant correlation between the physical and the psychic was attributed to the ceaseless action of the Deity.

This idea of God's mediation between mind and

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[1] Chauvin: Thesaurus Philosophicus, Art. Memoria, Rotterdam, 1692.

[2] Descartes: De Passionibus, I, XLII.

matter was developed by Geulinx and Malebranche into the doctrine of occasional causes. On occasion of a physical process, God calls forth an idea in the mind. On occasion of a volition, God moves the body. This metaphysical doctrine must be borne in mind, especially in studying Malebranche's views of memory, or one may be tempted to see in them more than he meant to put there; for in some passages he writes in the style of modern psychology.[1] He treats the correlation (*liaison*) of ideas with traces in the brain and the correlation or association (*liaison*) of the traces with each other. The cause of the association of the traces in the brain, and of the corresponding ideas, is identity of time when the impressions were made. Traces impressed upon the brain simultaneously are revived together: for paths of association are opened between traces made at the same time, and the animal spirits can pass along these paths more easily than into other parts of the brain. Again, some of the traces are *naturally* associated one with another and with certain emotions, on account of an arrangement of the fibres that we have had from birth. Malebranche notices also the importance of the association of ideas in morals, politics, and all the sciences relating to man.

Malebranche's view of the physiological processes connected with memory is similar to that of Descartes, and he deems the comprehension of the truth that all our varied perceptions are dependent upon cerebral changes sufficient for the explanation of memory. For "the fibres of the brain having once received certain impressions by the course of the animal spirits and by the action of objects, retain for some time a facility for receiving the same modifications. Now

[1] Cf. Recherche de la Vérité, II, v, passim.

the memory consists merely in this facility, since one thinks of the same things when the brain receives the same impressions."[1] The similarity of memory and habit as far as physiological processes are concerned did not escape the notice of Malebranche. In a sense memory is a kind of habit, and apart from consciousness there would be no difference between it and the other habits.[2] Making due allowance for metaphysical interpretations and the Cartesian hypothesis of animal spirits, such teachings show that Malebranche was a pioneer in the field of physiological psychology.

Spinoza's doctrine of memory is not very different from that of Malebranche, in spite of the difference in their philosophical systems. He is brief upon the subject, but explains the way the thought of one thing suggests that of another, and gives the essentials of the modern doctrine of the association of ideas. Memory depends upon this association of ideas; "for it is nothing else than a concatenation of ideas implying the nature of things outside the human body, which is formed in the mind according to an order and concatenation of physical states."[3]

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[1] Op. cit., II, v, 3.

[2] Op. cit., II, v, 4: Il est visible, par ce que l'on vient de dire, qu'il y a beaucoup de rapport entre la *mémoire* et les *habitudes*, et qu'en un sens la mémoire peut passer pour une espèce d'habitude. Car, de même que les habitudes corporelles consistent dans la facilité que les esprits ont acquise de passer par certains endroits de notre corps, ainsi la mémoire consiste dans les traces que les mêmes esprits ont imprimées dans le cerveau, lesquelles sont cause de la facilité que nous avons de nous souvenir des choses. De sorte que s'il n'y avait point de perceptions attachées aux cours des esprits animaux, ni à ces traces, il n'y aurait aucune différence entre la mémoire et les autres habitudes.

[3] Ethices, Pars II, Prop. XVIII.

X.—*Views of Early English Writers and of Leibnitz.*

Bacon writes sensibly of memory, giving special attention to the "Helps of memory."[1] He speaks slightingly of the mnemonic art as practiced in his day, esteeming the mnemonic feats of its devotees as no better than the "tricks and antics of clowns and rope-dancers." He was convinced that there might be better precepts and a better practice of the art than those in vogue. The art of memory, according to Bacon, is built upon what he calls Pre-notion and Emblem. Order, artificial places, and verse aid memory by giving a pre-notion of what the thing is we would recall. If we try to recollect a thing and have no pre-notion of what we would recall, "we seek and toil and wander here and there, as if in infinite space." But a pre-notion cuts off infinity and limits the range of memory. Emblem reduces intellectual conceptions to sense-images. We can remember an object of sense more easily than an object of the intellect. Hence the advantage of associating what is to be remembered with an emblem.

Memory has an important place among the faculties. In his psychology the three great powers of the human mind are Memory, Fancy, and Reason. Corresponding to this division of the faculties, he makes his famous threefold division of the sciences into History, Poetry, and Philosophy.

There is little new in the psychology of memory taught by Hobbes.[2] It is essentially that of Aristotle. All our knowledge originates in sense. The cause of sensation is physical motion. Memory and imagination are "decaying sense." "This *decaying sense*,

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[1] Cf. especially De Aug., V, 5; Nov. Org., II, 26.

[2] Cf. Human Nature, passim.

when we would express the thing itself, I mean *fancy* itself, we call *imagination* . . . but when we would express the decay, and signify that the sense is fading, old, and past, it is called *memory*." Thus memory and imagination are one thing with different names. Again, he terms remembrance a sixth sense, because it is concerned with the past. Names according to him are chiefly mnemonic devices. And he writes in Aristotelian fashion upon the association of ideas.[1]

The next English philosopher to be considered is Locke. He uses metaphorical language in discussing memory, speaks of the "repository of the memory" and the "constant decay of all our ideas, even of those which are struck deepest," says that when the ideas are not renewed "the print wears out," that "the pictures drawn in our minds are laid in fading colors," that "our minds represent to us those tombs to which we are approaching, where though the brass and marble remain, yet the inscriptions are effaced by time and the imagery moulders away," that in recollection our ideas are often "roused and tumbled out of their dark cells into open daylight by turbulent and tempestuous passions." But these are merely figures of speech. The psychology of Locke is very different from that of the Cartesians or that of Leibnitz. Not only has the mind no innate ideas, but it has no unconscious ideas. The pith of his doctrine of memory is expressed in the following passage: "But our ideas being nothing but actual perceptions in the mind, which cease to be anything when there is no perception of them, this laying up of our ideas in the repository of the memory signifies no more but this, that the mind has a power in many cases to revive perceptions which it has once

[1] Human Nature, IV, 2; also Leviathan, Ch. III.

had, with this additional perception annexed to them, that it has had them before. And in this sense it is that our ideas are said to be in our memories, when indeed they are actually nowhere, but only there is an ability in the mind when it will to revive them again, and as it were paint them anew on itself, though some with more, some with less difficulty ; some more lively, and others more obscurely." Locke distinguishes in the general faculty of *retention* two kinds of activity : First, the keeping of ideas for some time before the mind. This is *contemplation*. Second, the reviving of ideas without the help of the objects which first caused them. This is *memory*. Attention, repetition, pleasure and pain are the means of fixing ideas.

Locke does not discuss the physiological aspects of memory, but, from pathological phenomena, concludes that the constitution of the body may influence the memory. And from observation of animals, especially of birds learning tunes, he infers that several other animals as well as man have the faculty of retention. His chapter upon the association of ideas is a suggestive discussion of the influence of the habitual union of ideas upon the opinions, reasonings, and actions of men.[1]

Leibnitz not only opposed the empiricism of Locke and argued for innate ideas, or at least natural dispositions and tendencies, but he combated the notion that by memory we mean merely the faculty of recalling ideas at pleasure, the ideas having no existence when not before the mind. "If nothing remained," he says, "of past thoughts as soon as we cease to think of them, it would not be possible to explain how

[1] Essay concerning Human Understanding, Book II, Chaps. X, XIX, XXXIII.

one retains the memory of them; to have recourse to that bare faculty for explanation, is to say nothing intelligible." Leibnitz favored rather the Cartesian hypothesis, that the conditions of memory are traces or dispositions left in the soul as well as in the body by former impressions. These traces remain, though we are unconscious of them; and also the effects of things we cannot recall may remain in the mind.[1]

Wolff, the disciple of Leibnitz, makes memory a faculty of the soul, and the Leibnitzo-Wolffian psychology prevailed in Germany until Kant.

XI.—*Conceptions of Memory in the* 18*th Century.*

The historical study of memory has more than a psychological interest. The subject is connected with some of the profoundest questions of philosophy. About it materialists against idealists, and empiricists against nativists, have fought their battles. The doctrine of the Cartesians, that memory was conditioned by traces left in the brain, was developed by the physiologists and Encyklopedists of the 18th century into a materialistic and mechanical view of memory. Sometimes the help of mathematics was sought to make the mechanical view more definite. The physiologist Haller performing the first experiments upon the time occupied in psychic processes, had estimated that a third of a second was sufficient time for the production of one idea. On this basis Hook and others reckoned that in a hundred years a man must collect 9,467,280,000 traces or impressions of ideas in his brain. Reducing this to a third on account of sleep, etc., one has 3,155,760,000, or in fifty years 1,577,880,000 traces. Assuming the weight of the brain to be four pounds,

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[1] Nouveaux Essais, I, iii. 18, and II, x. 2.

and deducting one pound for the weight of the vessels and blood, and another for the weight of the cortex (which, strangely enough, they thought did not have the power of preserving impressions), 205,542 traces must be found in one gram of the nerve-substance of the brain.[1] Haller, however, while basing memory upon traces in the brain, admitted his ignorance of the nature of these traces, and opposed materialism.

Condillac is, perhaps, the best representative of the psychology of the Encyklopedists. In his famous statue gradually endowed with sense he traces the development of memory and imagination.[2] When the statue is endowed with the sense of smell, he says that this supposed man would have no knowledge of the relation of things without memory. He would suffer and rejoice without having either desire or fear. "But the odor which the statue perceives does not entirely escape from it as soon as the odorous body ceases to act upon its sense-organ. The attention bestowed upon the sensation retains it still; and there remains an impression, more or less strong according as the attention itself has been more or less active. This is memory."[3] When the statue perceives a new odor, that which it had a moment before is still present. The power of perceiving is divided between the remembered sensation and the present odor. The statue compares the two and learns to judge. By repeated exercise, acts of memory, comparison, and judgment become habitual. At the first sensation experienced, the statue has no surprise, for it has been accustomed to no other sensation; but when it passes from an

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[1] Cf. Huber: Das Gedächtniss, p. 21.

[2] Traité des Sensations, passim.

[3] Op. cit., Part I, Ch. II, §6.

accustomed to a totally different state, then it experiences surprise. Surprise arouses the attention. If the odors equally attract the attention, they are stored in memory in the order of their succession. If there are a great many impressions in the succession, then the last and the most novel will be the strongest. In memory, then, we have a series of ideas which form a kind of chain. This linking of the ideas furnishes the means of passing from one idea to another and of recalling the most distant. The great law of association of ideas is coexistence in time. Two degrees in the power of recollection may be distinguished: one weak, where a thing is recalled as past; the other strong, where a thing is recalled so vividly that it seems to be present. The one is called memory, the other imagination. These two faculties differ only in degree. Memory is weak imagination. Imagination is the most vivid kind of memory.[1]

The great mystery of memory, Condillac treats as follows. Where, he asks, is the idea of a thing when for a long time the mind does not think of it? It is not in the mind; for disease can destroy the power to recall it. It is not in the body. Only a physical cause could preserve it there; and it would be necessary to suppose that the brain remains in exactly the same state in which it was put by the sensation which the statue would recall. Moreover, it would be difficult to reconcile this hypothesis with the continual movements of the animal spirits, and with the multitude of ideas with which memory is enriched. Condillac gives what he deems a simpler explanation. "I have a sensation," he says, "when a movement occurs in one of my organs and is transmitted to the brain. If the

[1] Op. cit., I, ii. 29.

same movement begins in the brain and is propagated to the sense-organ, I believe that I have a sensation that I do not have: it is an illusion. But if this movement begins and ends in the brain, I remember the sensation that I have had. When an idea returns to the statue, it is not that it has been preserved in the body or in the mind: it is only because the movement which is the physical and occasional cause of it, is reproduced in the brain."[1]

Although, for the sake of simplicity, Condillac first endows his supposed statue with the sense of smell, and traces the development of memory while it has this one sense, he looked upon the sense of touch as the basis of all the ideas we retain in memory, and thought the memory of the ideas which arise from touch stronger and more enduring than that of ideas coming from the other senses.[2]

Helvetius considers a few special points.[3] He attempts to show that a great intellect does not necessarily imply a great memory, and concludes that, on the contrary, extreme capacity of the one is exclusive of great capacity of the other. The relation of memory to the intellect is expressed in the following sentence: "Memory is the storehouse where are deposited sensations, facts, and ideas, the various combinations of which form what we call intelligence (*esprit*)."

Helvetius considers that memory is almost entirely factitious. The great differences in memory among educated men are due less to differences of natural endowments than to different degrees of training. Men with feeble memories, like St. Augustine and

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[1] Op. cit., I, ii. 38.

[2] Op. cit., Partie II, Ch. XI.

[3] Discours de l'esprit; disc. III, c. 3.

Montaigne, learned much on account of their great desire to learn. The capacity of memory depends upon three things—on the daily use of it, on attention, on the order in which one arranges one's ideas. He emphasizes the last. A great memory is, as it were, a phenomenon of order.

Helvetius further attempts to prove that all men are endowed with sufficiently good memories to enable them to attain a high degree of intellectual culture. "Every man," he argues, "is really sufficiently favored by nature in this respect, if the storehouse of his memory is capable of containing such a number of ideas or facts that by constantly comparing them he can always perceive some new relation, always increase the number of his ideas, and consequently always increase the capacity of his intellect. Now, if thirty or forty objects, as geometry shows, can be compared in so many ways that, in the course of a long life, no one can observe all their relations nor deduce all the possible ideas from them; and if among men whom I call well endowed (*bien organisés*), there is no one whose memory cannot contain, not only all the words of a language, but also an infinity of dates, facts, names, places, and persons, and, finally, a number of objects considerably more than six or seven thousand, I conclude confidently that every well-endowed man is given a capacity of memory far beyond what he can make use of for increasing his ideas; that greater capacity of memory would not give greater capacity of intellect; and thus that, instead of regarding the inequality of memory in men as the cause of the inequality of their intellects, this latter inequality is entirely the result, either of the attention, greater or less, with which they observe the relations of objects,

or of the bad choice of objects with which they load their memories. There are, indeed, barren objects, and those, such as dates, names of places, persons, and other like things, which occupy a large place in the memory without being able to produce either a new idea or one interesting to the world . . . . This is why one is seldom a great man who has not the courage to be ignorant of an infinite number of useless things."[1]

One of the greatest physiologists and thinkers of the eighteenth century was Bonnet. His views of memory are much like those of Condillac.[2] Yet he opposes materialism, and claims that man is no more all matter than all spirit; he is rather an *Etre-mixte*. Bonnet's method was, he says, to look for the immediate antecedents of a thing. Before searching for the way an idea was reproduced, he inquired how it was produced. All our ideas, according to Bonnet, are derived from sense. The kind of sensation depends upon the anatomical structure of the sense-organ. Not only do the different sense-organs have different nerve-structure, but the fibres of the same sense-organ vary in structure.[3] The phenomena of refrangibility of the rays of light and the vibration of the cords of sonorous instruments strengthened this conjecture. "Each perception," he says, "has its character which distinguishes it from every other. For example, each ray of color has its essence, which is invariable. A red ray does not have

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[1] Discours de l'esprit; disc. III, c. 3.

[2] Cf. Contemplation de la Nature, Part V, Ch. 6; Essai de Psych., Ch. 4, 5, 6, 27, 31; Essai analytique sur l'âme, Ch. 7-9; Analyse abrégée de essai analytique, ¶ 7-11; Essai d'application des Principes Psych. de l'Auteur, passim; Philalethe, Ch. 3; Palingénésie, Part II, Ch. 1.

[3] Analyse abrégée, ¶ IX: "Si chaque sens a sa mechanique, j'ai cru que chaque espèce de fibre sensible pourrait avoir la sienne."

precisely the same effect as a blue ray. There are then also, among the sight fibres, differences corresponding to the differences in the rays."[1]

The physical correlative of a sensation is a movement or vibration in the fibres of the sense-organ. The reproduction of a sensation likewise depends upon a physiological process. Memory and imagination have their seat in the body. This is proved by pathological cases in which accidents affecting only the body weaken and destroy these powers. Anticipating Bain, he says that the reproduced impression depends upon the vibration of the same nerve-fibres as transmitted the original impression to the mind.[2] The fibres that transmit and reproduce impressions have a structure adapted to this double function. "The sense-fibres are constructed in such a way that action more or less prolonged of objects produces in the fibres determinations more or less durable." Admitting his ignorance of the structure of the sense-fibres, Bonnet did not venture to explain the nature of these determinations.

His general doctrine of the physics of memory is well expressed in the following passage: "Not only does the fibre transmit to the mind the impression of the object; but it also retraces the memory of that impression. This memory differs from the sensation itself only in the degree of intensity. It has then the same origin; it then, as well as the sensation, depends upon

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[1] The doctrine of specific nerve energy here expressed is noteworthy. Cf. also Contemplation de la Nature, Tome I, pp. 97 and 98, 2d ed., Amsterdam. "Chaque sens renferme donc probablement des fibres *spécifiquement* différentes. Ce sont autant de petits sens particuliers, qui ont leur manière propre d'agir, et dont la fin est d'exciter dans l'âme des perceptions correspondantes à leur jeu."

[2] Analyse abrégée, ¶ IX: "C'est à l'ébranlement de certaine fibres, que cette sensation a été originairement attachée. Sa *réproduction* ou son rappel par l'imagination, tiendra donc encore à l'ébranlement de ces mêmes fibres."

a movement excited in the fibre, but a feebler movement. The execution of this movement demands a certain disposition in the constituent parts of the fibre. The elements retain then, for a longer or shorter time, the determinations which they have received from the action of the object. It strings the fibre, so to speak, to its tone; and, as long as the fibre remains thus strung, it preserves a tendency to retrace to the mind the memory of the sensation from the object."[1] From the passage just quoted we see that, when a vibration has once occurred, a certain flexibility imparted to the nerve makes it easier for the same vibration to occur again than for a new movement to take place. This increased facility of vibration is the cause of our recognizing a sensation or an idea when it occurs a second time.[2]

The connection between habit and memory, which Malebranche had so well pointed out, did not escape the observation of Bonnet. He saw, too, that the nerves depend upon nutrition, and that they retain in growth their functional dispositions. To quote his words: "The sense-fibres depend upon nutrition like all the other parts of the body; they assimilate or incorporate alimentary substances; they grow; and, while receiving nutriment and growing, they perform their peculiar functions, and remain in essence unchanged. Their mechanism is, then, such that they incorporate nutriment in direct conformity with their structure and their acquired tendencies. Thus nutrition tends to preserve in the fibres these tendencies and cause them to take root; for they increase in stability in proportion as the fibres grow, and, I believe, we see here the

[1] Analyse abrégée, ¶ X.

[2] Cf. Essai de Pysch., Ch. V; and Essai an. sur l'âme, Ch. IX.

origin of habit, that powerful queen of the sentient and intelligent world."[1]

The views of Bonnet, probably more than of any other philosopher we have studied, have the ring of modern physiological psychology. He announces clearly that psychic processes have their correlative in physical processes. The brain is the organ of mind. And the tenacity of memory depends upon the ability of the brain elements to retain determinations imparted to them. As the tendencies to particular modes of vibration preserved in the brain were caused by the action of external objects, it is, in a sense, a mirror of a portion, larger or smaller, of the universe. In writing of this Bonnet grows eloquent. "What images," he exclaims, "are those in the brain of a Homer, a Virgil, or a Milton! What mechanism executes those marvellous scenes! The intelligence which could have read in the brain of Homer, would have seen the Iliad represented by the varied play of a million fibres."[2]

It should be noticed especially that Bonnet was not a materialist. He was, however, ready to accept any results that investigation might furnish. His fearless attitude toward materialism may be inferred from the following passage: "If some one should ever demonstrate that the soul is material, far from being alarmed at it, one should wonder at the power which had given matter the ability to think."[3]

Among English philosophers of the eighteenth century, Hume has something to say about memory and the association of ideas.[4] He reverses the distinction

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[1] Contemplation de la Nature, Tome I, pp. 99 and 100, 2d ed.
[2] Ibid.
[3] Analyse abrégée, ¶ XIX.
[4] Cf. especially Treatise of Human Nature, I, iii. 5; I, i. 3 and 4.

that Condillac makes between memory and imagination. The difference lies in the "superior force and vivacity" of memory. "A man may indulge his fancy in feigning any past scene of adventures; nor would there be any possibility of distinguishing this from a remembrance of a like kind, were not the ideas of the imagination fainter and more obscure."[1] The ideas of memory often degenerate and we are unable to distinguish them from the ideas of fancy. On the other hand, illusions of memory are possible. "An idea of the imagination may acquire such a force and vivacity as to pass for an idea of the memory, and counterfeit its effects on the belief and judgment. This is noted in the case of liars, who, by the frequent repetition of their lies, come at last to believe and remember them as realities; custom and habit having in this case, as in many others, the same influence on the mind as nature, and in fixing the idea with equal force and vigor."[2]

The laws of association according to which simple ideas are united into complex ones are three in Hume's psychology—*resemblance, contiguity* in time or place, and *cause* and *effect*. The last is most extensive. Even here, however, there is no necessary connection; but the idea of causality is the result of an experience of uniform sequence. Of association he says in a famous passage: "Here is a kind of *attraction*, which in the mental world will be found to have as extraordinary effects as in the natural, and to show itself in as many and various forms."[3]

Hume discusses the relation of memory to the prob-

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[1] Op. cit., I, iii. 5.
[2] Ibid.
[3] Op. cit., I, i. 3.

lem of personal identity. Memory is the chief source of personal identity, because it makes known to us the extent and continuity of the succession of our perceptions. But it does not produce, it rather *discovers* to us personal identity, by showing us the relation of cause and effect among our perceptions. "Who can tell me," asks Hume, "what were his thoughts and actions on the first day of January, 1715?" Nevertheless, though one cannot recall what he did on a given date several years in the past, yet no one doubts the identity of that past self with his present self. Thus Hume argues: "It will be incumbent on those who affirm that our memory produces entirely our personal identity, to give a reason why we can thus extend our identity beyond our memory."[1]

Hartley, the eminent English physiologist, in accordance with the general principles of his psychology, outlines an interesting form of the vibratory theory of memory[2] not essentially different from that of Bonnet. Influenced by Newton, he believed that ether pervades all things, even the most solid bodies. In sense-perception, vibrations imparted by the ether are transmitted by the nerves to the brain and there transmuted into sensations. The sensation is the subjective aspect of the vibration. When the sensory vibrations cease, dispositions to diminutive vibrations persist in the medullary substance of the brain. These diminutive vibrations, the physical correlatives of decaying sense, Hartley calls vibratiuncules. They are the condition of our ideas. The vibratiuncules from sensations that were simultaneous or successive become associated, so that we have clusters of impressions and complex

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[1] Op. cit., I, iv. 6.

[2] Cf. Observations on Man, Sect. IV., Prop. XC.

ideas. In memory the vibratiuncules are renewed, and the same ideas and clusters of ideas are revived.

Memory depends entirely upon the state of the brain. For it is impaired or destroyed by diseases, concussions of the brain, spirituous liquors, and the like, and generally returns with the return of health. If sensations and ideas arise from vibrations and dispositions to vibrate in the medullary substance of the brain, it is easy to see that these causes would disturb the order of ideas.

From the subjective standpoint Hartley defines memory as "that faculty by which traces of sensations and ideas recur or are recalled in the same order and proportion, accurately or nearly, as they were once actually presented."[1] Thus memory is based upon the association of ideas. The great law of association is that of contiguity in time or space. Hartley considered at great length the association of the vibratiuncules, and of the corresponding ideas, attempting to show that all reasoning and affection are the result of association. He at least succeeded in laying the foundation of the modern Associational Psychology.[2]

Hartley considers the various phenomena of memory, such as the defects of memory in children, old people, and diseased persons, and tries to make them tally with his theory. According to his psychology, memory is a fundamental power of the mind. All our voluntary powers are of the nature of memory. The results of observation in pathological cases agree with this; for in diseases of memory the voluntary actions

[1] Op. cit., Introduction, p. 2.

[2] For a good history of the doctrine of association of ideas see Ferri: La Psych. de l'Association depuis Hobbes jusqu'à nos jours; also for older views, Hissmann: Geschichte der Lehre von der Association der Ideen; Göttingen, 1777.

are imperfect. Taking memory in a large sense, all the powers of the soul may be referred to it. Thus strong power of retention is indispensable to strong judgment; and though some persons with weak judgments may have strong memories, no one with a weak memory can have a strong judgment.

It is interesting to note, in connection with such studies as those recently made by Kraepelin,[1] that Hartley, as well as Hume and others, noticed the possibility of illusions of memory. The difference between memory and reverie consists, he thinks, in the greater vividness of the clusters of memory pictures, and principally in the readiness and strength of the associations by which they are united. Many persons, he points out, are known by some false story that they relate over and over. By magnifying the ideas and associations they at last come to believe that they remember what they tell. The story makes as vivid an impression on them and hangs as closely together as any assemblage of past facts in their memory. Thus "all men are sometimes at a loss to know whether clusters of ideas that strike the fancy strongly, and succeed each other readily and immediately, be recollections or mere reveries. And the more they agitate the matter in the mind, the more does the reverie appear like a recollection." As when in endeavoring to recollect a verse, a wrong word suiting the place, and afterward the right one occurs, one sometimes becomes confused, and for the moment it is hard to distinguish the right one. "Persons of irritable, nervous systems are more subject to such fallacies than others. And madmen often impose on themselves in this way, viz., from the vividness of

[1]Archiv f. Psychiatrie, 1886 and 1887.

their ideas and associations, produced by bodily causes. The same thing often happens in dreams. The vividness of the new scene often makes it appear like one that we remember and are well acquainted with."

XII.—*Mnemonic Systems.*

No historical sketch of the doctrines of memory among the older writers would be complete without some mention of their mnemonic systems. The art of mnemonics seems to have been much in vogue among the ancient Greeks and Romans. Every scholar of the classics is familiar with the story that ascribes the invention of the art to Simonides. There are allusions to this art in the works of Aristotle, Plato, and other classic writers. Aristotle is reported by some to have written a work upon mnemonics. Cicero and Quintilian give special attention to the subject.[1]

The main principles of the ancient mnemonic systems were somewhat as follows: The thing to be remembered was localized by the imagination in some definite place—say in a room of a real or imaginary house; and, if necessary, a concrete symbol as vivid as possible was associated with it. Often a large house was visualized in the imagination, and the rooms, walls, furniture, statues, etc., associated with things to be remembered. To recall anything it was only necessary to rummage about in this imaginary house until one found what was desired. This device was much used among the Romans as an aid to oratory; and it has been said that the phrases, *in the first place, in the second place*, and the like originated in this ancient practice.

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[1] Cicero: De Oratore, II, 86–88; Rhet. ad Herenn., III, 16–24. Quintilian: Inst. XI, 2. Cf. also Pliny: Hist. Nat., VII, 24.

From the fifth to the thirteenth century the mnemonic art may have been practiced in the monasteries, but we hear little of it. In the thirteenth and fourteenth centuries the ancient systems were revived. Roger Bacon was one of the writers upon the subject; but his work was never printed, though it is said to be still preserved in MS. at Oxford. Toward the close of the fifteenth century the famous teacher, Petrus de Ravenna, appeared, and the first edition of his *Ars Memorativa* was published in 1491. In the sixteenth and seventeenth centuries a great many books upon mnemonics were published. Among the most important were the works of Lamprecht Schenkel and Giordano Bruno. Winkelmann and Leibnitz invented, or borrowed from the Hebrew Bible, the device of representing figures by letters. And later Grey made special application of this principle in his *Memoria Technica* which appeared in 1730.

The character of some of the mnemonic teachers of this period may be inferred from the following passage from Cornelius Agrippa's *De Vanitate Scientiarum.* Speaking of the vanity of the mnemonic art where there is not a good natural memory to begin with, and of the authors who have written upon the subject, he says: "Many there be that at this day profess the same, though they get more infamy and disrepute than gain thereby; being a sort of rascally fellows that do many times impose upon silly youth, only to draw some small piece of money from them for present subsistence."[1] There is at least this difference between the mnemonic teachers of Agrippa's time and those of the present. The latter generally get, not a small

[1] The Vanity of Arts and Sciences; Eng. translation: London, 1676. For a modern example of the mnemonic money-getter, see "*Loisette*" *Exposed,* by G. S. Fellows; New York, 1888.

piece of money, but a large piece, and they sometimes impose upon others as well as silly youth.

Most of the systems taught before the time of Grey seem to have differed little from the ancient systems. Localization and visualization were the characteristics of them. Sometimes mnemonic towns, with numerous streets, squares, and buildings, were formed. By continued thought the mnemonic expert became at home in this imaginary town. It was laid out probably according to the classification of the sciences, and by this device the abstract was associated with the concrete, and the imagination brought to the aid of memory.

The ancient and mediæval systems of mnemonics are inferior to the best modern systems, especially that of Pick,[1] which is based upon sound psychological principles. But they were probably very helpful to eye-minded people. The men with remarkable memories mentioned by Cicero and others probably owed much to mnemonic aids. It is of special psychological interest to consider the ancient mnemonic devices in connection with such studies as those of Galton upon mental imagery, number forms, and the like.[2] The prevalence of these systems may indicate that the faculty of visualization was highly developed in many of the ancient Greeks and Romans, and among the devotees of their mnemonic systems in the Middle Ages.[3]

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[1] Pick: Memory and the Rational Means of Improving it. London, 1861.

[2] Enquiry into Human Faculty, p. 83 seq.

[3] Ersch and Gruber mention some 140 works upon mnemonics; see their *Algemeine Encyclopädie*, art. *Gedächtnisskunst*. Pick says that Aimé Paris gives a list of 300 works on memory and on mnemonics. G. S. Fellows, in "*Loisette*" *Exposed*, gives a bibliography of 247 works on mnemonics and the training of memory. Cf. also on the history of mnemonics, Enc. Brit., art. Mnemonics; Pick, Memory

XIII.—*Conclusion.*

The material contained in the foregoing pages, meagre though it is, may be taken as fairly illustrative of the conceptions of memory that have prevailed from the earliest times until the great era in philosophy marked by the appearance of the Critique of Pure Reason. Whether or not we agree with Emerson that all men may be divided into Platonists and Aristotelians, the various theories of memory studied naturally divide into two series—one begun by Plato, the other by Aristotle—the former transcendental, the latter physiological and empirical in its tendency. Plato, the Neo-Platonists, St. Augustine, Leibnitz regard memory as an act of the soul, limited, perhaps, by physiological processes, but not dependent upon them. Sensation may furnish memory the data in great part, yet memory belongs not to the sensory but to the intellectual part of the mind. On the other hand, according to Aristotle, Thomas Aquinas, Hobbes, Condillac, Bonnet, and others (making allowance for differences of opinion due to their individual systems), memory belongs to the sensory side of the mind. The images of memory and the imagination are the relics of former sensations. The sensations were due originally to physiological processes. The reproduced images depend upon physiological processes, weaker, but not essentially different from the original ones.

The theories of memory that we have studied may be of little value in themselves, but they form a part of the data necessary for a complete study of the psychology of memory. These theories were formed

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and its Doctors; Aretin, *Systematische Anleitung zur Theorie und Praxis der Mnemonik,* Sulzbach, 1810; Middleton, Memory Systems New and Old.

very much as we form our theories to-day, *i. e.*, by generalization from observed facts—with less of scientific rigor probably, with the usual coloring of the thinker's mental environment, and with the peculiar ornaments of the individual apperception. But, if "million-eyed observation" is better than the observation of any one man, if the experience of the race is more trustworthy than that of the individual, then a theory, though worthless as such, may be valuable because containing, however obscurely, a record of the observation and experience of the times when it was formed. Most of all, however, a theory, worthless in itself, may be valuable as an instance of the working of the human mind before one of the greatest problems of psychology. A great number of such instances may prove valuable for psychological study in the same way as the myths of savage tribes and the records of child-life.

Aristotle's doctrine of memory, for example, as a theory is partly false; but it is a remarkable instance of the tendency of the human mind to find satisfaction in resolving all mysteries back to the one supreme mystery of motion. This conception of motion played its part, too, in the systems of the Middle Ages. Hobbes made motion the basis of his system. The motion of the animal spirits was the occasion of psychic activity according to the Cartesian psychology. Memory as reproduced movement was the theory that gave most comfort to Condillac. And with Bonnet and Hartley memory is the result of persisting vibrations in nerve-substance. This theory has been taught, in one form or another, by many physiologists ever since. That so many thinkers have found the explanation of memory in motion is profoundly suggestive. The

human mind has a passion for unity. If it cannot solve all its difficulties, it likes to collect them under one all-embracing mystery. This appeases the desire for unity, and economizes energy. Psychologically considered, the category of motion is an economic device that satisfies the Aristotelian mind.

Equally worthy of study are those theories that see in memory an activity independent of physiological processes, a transcendental function of the timeless and spaceless intellect.

In recent years the subject of memory has broadened. It is now connected with some of the most profound questions of psychology and biology. As the knowledge of these sciences has advanced, the importance of the study of memory has increased. Yet it is noteworthy that the beginning of the newer views is found in the doctrines of the older writers studied in this article.

THE PLACE FOR THE STUDY OF LANGUAGE IN A CURRICULUM OF EDUCATION.

M. PUTNAM JACOBI, M. D.

The study of language has always occupied a conspicuous place in educational curricula. The Greeks, who counted all languages but their own barbarian, taught the grammar of their own as the basis of all education. The Roman children studied Greek as ours do French—less as an education than as a fashion. The first mediaeval schools established grammar in the trivium, or most elementary course, and also in the quadrivium. The feeling has always prevailed in civilized communities, that as the mind was never seen to work without language, the study of language must lie at the basis of all mental training. We know now that much mental action precedes the use of words, and whenever we are logical to the laws of mental development, we train the mind to handle sense perceptions of external objects before we introduce the systematic study of language, even in reading and writing the mother tongue. Every one knows, however, that this change in the school curriculum is most recent.

The moment arrives at last when the study of language must begin, even if nothing is learned but the native language of the child. This moment may to a certain extent be compared with that illustrious epoch in European history, when at the Renaissance of learn-

ing, classical Latin and Greek were rediscovered for the modern world. The extraordinary effect of this discovery may well serve to prove the importance of language to thought. With an imperfect and inadequate language, the nations of Northern Europe had remained in a narrow, cramped, and as we now often say, with perhaps considerable exaggeration, a barbarous existence. Restored to the noble speech of which they were the just inheritors, their compressed life rapidly expanded to its measure. The new vitalities aroused, soon in turn expanded the hidden potentialities of the antique tongues to all the flexible and varied needs of the modern life, and this life rapidly developed to a hitherto unknown degree of complexity. An immense number of thoughts seemed to have been impossible from the lack of fitting words. When these words were found—the buried treasure of bygone ancestors—the thoughts sprang to them as rider to the saddle; and with new ideas, life was regenerated.

Thus, although the material for the physical sciences existed in the same abundance then as now, these sciences failed to develop until after the Renaissance of classical learning. It seemed necessary that Scaliger and Erasmus, in the sixteenth century, should precede Gilbert and Harvey in the seventeenth, to render possible their discoveries of electricity and of the circulation of the blood. The solitary labors of Roger Bacon in the thirteenth century had flickered like a taper in a vast cavern of darkness, and then failed for lack of air. The human brain could not advance in analysis of the external world until it had been disciplined and developed in its internal activity by training in language.

But, at the present day, the educational value of the

study of languages has begun to be seriously questioned. In a late number of the *Forum* Dr. Flint declares that as much mental discipline can be obtained from study of physics and chemistry as from study of languages, and that the knowledge thus gained is both more useful and more easily understood than the construction of Latin and Greek. He also observes that the range of subjects on which knowledge is desirable has greatly widened since the classical curricula were planned, and that it is impossible to do justice to all that is necessary to-day if we continue to fulfil all the demands which were made two hundred years ago. Similar remarks are repeated over and over again, and on all sides. These assertions touch, indeed, upon some truth, but they do not comprehend all of it, and they overlook much that is essential to the questions at issue. The problems to be considered are :

1. Does the study of language exercise any different effect upon mental development from the study of any other subject, and if so, what is it ?

2. How does the effect of language study compare with that of mathematics, of the physical sciences, of the moral and historical sciences ?

3. If such special effect can be proved, at what age or epoch of education is it most appropriate and useful to seek for it ?

4. Is there any difference between the effect on the brain of the classic and the modern languages ?

5. If languages are to be taught, how is the necessary time to be secured for teaching other things most important to know and too often neglected ?

6. What proportion should these relative branches of study bear to each other in a general, non-specialized curriculum ?

7. What special devices or methods may be suggested to facilitate the accomplishment of the above mentioned ends?

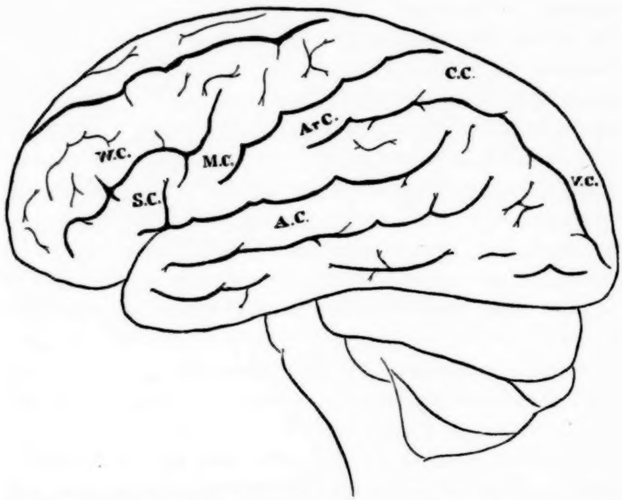
At the outset I would call attention to a fact which might seem self-evident, yet is generally overlooked in pedagogical discussions of the subject. This is, that the study of languages must be an extension, more or less complex, of the process of acquiring language—the highest physiological acquisition that distinguishes the human race from the lower animals. The method and educational results of such study are, therefore, primarily a physiological problem, and should be discussed by physiologists before they are handed over to pedagogues.

The genesis of speech is one of the most extraordinary and mysterious phenomena in the history of mankind. It has always justly excited the astonishment and speculations of philosophers.

It is most difficult to understand why any particular sound, or group of sounds, should have become significant of one object or idea rather than of any other. A purely physiological theory has tried to classify all words according to the parts of the articulating apparatus at which their fundamental sounds are formed, thus giving one intrinsic meaning to guttural sounds, another to labials, another to dentals, etc. But this theory cannot meet all the facts of the case. Prof. Max Mueller, who traces all words in the Indo-European languages back to 850 primary Sanscrit roots, is inclined to accept another physiological explanation of the genesis of the roots. This is the theory of Noiré. The latter has pointed out that whenever a number of people are engaged together in any muscular work, they have a tendency to utter aloud certain rhythmical

sounds. "These are almost involuntary vibrations of the voice, corresponding to the more or less regular movements of the whole bodily frame." Noiré suggests that some special nerve element, or group of nerve elements, in the brain is then thrown into vibration coincidently with the external muscular movement; and this associated nerve vibration being propagated to the part of the brain which innervates the organs of articulation, the latter are excited to so modulate the simultaneously developed current of air in expiration, that a definite sound, one of the primitive root sounds, is produced. This verbal root remains associated with the act which was being performed during its articulation, and finally becomes an expressive sign for the entire class of acts during which it is habitually repeated. "Thus would be explained," observes Mueller, "the fact that the primitive Sanscrit roots all express actions and not objects: as actions of digging, cutting, rubbing, etc." Words expressive of other ideas are derived from the first by analogy and metaphor. This theory should also explain why any given root should bear a special relation to any given action, and hence come to express any special group of ideas. It does so, because it has been generated in a cerebral excitation, that has happened to coincide with such other cerebral excitations as have been necessary to the performance of voluntary muscular actions.

Thus, in the figure, *Ar. C.* is placed on a part of the brain that we know is always excited when a person is using his right arm; *S. C.*, on a point very near it, which is always excited when he is speaking. Nerve impulses pass from this point down through the brain until they reach the nerves coming from the base of it, and



W. C. writing centre, *S. C.* speech centre, *M. C.* motor centre for lips, *Ar. C.* arm centre, *C. C.* concept centre (location hypothetical), *A. C.* auditory centre, *V. C.* visual centre.

which go to the throat, tongue, palate and lips. According to the theory, the excitation or vibration of nerve elements at the point *Ar. C.* spreads to point *S. C.*, the so-called centre of articulation, where it throws nerve cells into some special form of vibration. This special form of vibration is transmitted out of the brain, along nerves going to the lips and other organs of articulation, and the current of air which is at the moment issuing from them is moulded into some special articulate sound. This becomes a root, an auditory sign, which first evolved (according to the hypothesis) during the performance of a given act, is repeated with every repetition of the act, and gradually becomes an abstract sign corresponding to the gen-

eralized conception of such a class of acts. Thus the first abstraction of speech would result from a generalized experience of a succession of personal actions. In the second stage of development, the sign would be extended by analogy to other actions than the original one; finally to the properties of objects that seemed explicable by reference to these actions, which were better known than the objects themselves.

Thus, observes Mueller, every root expresses a concept or general notion, or more correctly, the remembered consciousness of repeated acts, as scraping, digging, striking, joining, etc.[1] As a single illustration. From a root *khan*, to dig, easily came *khana*, meaning not only a digger, but also a hole ; and *khani*, a digger and a mine.

I will not dwell on the various interesting facts which might be adduced in support of this theory. But, in considering it, we are led to note the fundamental circumstance that speech implies a more extensive excitation of the brain than does any action performed without speech, including in the latter the systematized thinking which clothes itself in words. In its most rudimentary form, the articulate utterance accompanying a muscular movement implies that nervous action has spread from the nerve centres governing the movements of limbs, to those adjacent centres which control the organs of articulation. Closely adjacent to these centres are other portions of the brain which have no immediate connection with nerves either going to or coming from the brain. The Island of Reil is one of them. These portions of the brain are concomitantly drawn into the vortex of excitement, and when that is the case, the vibrations

[1] The Science of Thought, p. 214.

of nerve cells and fibres which occur during the utterance of the speech, are repeated or registered, as it is said, in these extra-sensory centres. It is then, in some mysterious way, that the consciousness or conception of speech is generated in the brain and mind of the speaking individual. The genesis does not occur unless the supra-sensual, superadded convolutions of the brain have attained a high degree of development, and this is why no animal but man is able to speak.[1]

When any one learns the terms of a fully developed speech, or a baby learns his own language, the process is different. Here is no question of generating a spoken sign, compelled to assume an indissoluble relation to some thing. But it is only necessary to learn the spoken sign already created, and the fact that it *is* associated with a thing.

The sound of the word, as bread, falls upon the air and causes a peculiar vibration of the nerve running from the ear to the brain—the auditory nerve. This vibration is transmitted to a special locality of the brain, apparently the first temporo-sphenoidal convolution. Now, if the child has never seen any bread, the sound, though registered, arouses no mental conception ; it seems to have no meaning. It is the same when an adult hears a word in a language to him unknown, or when the subjects of certain forms of brain disease hear words after they have lost the power of attaching any significance to them. But if the baby—to return to him—has seen a piece of bread ; if he has become sufficiently interested in it to notice the association of this verbal sign with it ; if the asso-

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[1] See the most interesting paper of Broadbent on "Cerebral Mechanism of Speech and Thought," Med. Chir. Trans. 1872.

ciation has been distinctly pointed out to him, by pronouncing the name at the same time that the bread is shown or given, then another process takes place in his brain. At the same time that the name is registered in this part of the brain, the receptacle for auditory impressions, a visual impression of the object is registered at another point—the cuneus, or posterior portion of the occipital lobes. Often, indeed, the visual impression has been made long before; the child has recognized the appearance of the piece of bread, when it could not as yet name it, but only reach after it with a gesture.

When the two impressions have been registered in the brain—the visual impression of the object, and the auditory impression of its name—they may then be combined. Exactly how this combination is effected we do not know; but we can represent to ourselves that vibrations, similar to those of the auditory nerve, are transmitted along the fibres which connect these two points of the brain. When this happens, a secondary vibration is coincidently transmitted in another direction to the convolutions "superadded" to the simplest ones which belong to the sense impressions. In these convolutions the more complex combined vibration becomes the material correlative of an ideal concept, composed of the reminiscence of the visual impression of the object and of the auditory impression of its name.[1] Taine remarks that a couple is then formed, either member of which is thenceforth

[1] L'Intelligence, p. 6. The precise statement is as follows: "In the formation of *couples*, such that the first term of each suggests the second term; and in the aptitude of this first term to stand wholly or partially *in place of* the second, so as to acquire either a definite set of its properties or all those properties combined, we have, I think, the first germ of the higher operations which make up man's intelligence."

able to draw the other into consciousness. The sound of the name suggests the image of the object; the sight of the object suggests the sound of the name.[1]

The association of written signs with visual images and with auditory signs is obviously only an extension of the same process, and need not be dwelt on here.

The child learns to recognize a word before he learns to use this word himself; but finally this step also is taken. He articulates the word "bread" under the influence of an internal impulse or desire composed of the sensation of hunger, of the reminiscence of the visual impression of the object, of other impressions or memories connected with it, as of its hunger-satisfying property; finally, of the auditory impression of its name. This complex internal impulse, when definitely formulated, corresponds to an excitation of some part of this intermediate portion of the brain that we may call for convenience, as it has been called, the concept centres. From these centres the excitation spreads to that point, whence are innervated the organs of articulation; and when they are excited in the proper way the child is uttering the word "bread." By that time an entire cycle of cerebral activity has been traversed, and the greater part of the area of the brain has been excited. *It is plain, therefore, that to learn the name of a thing, and to learn how to use this name, involves much more mental action than is required simply to acquire sense perceptions about it.*

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[1] In an interesting paper on Apraxia and Aphasia, by Dr. Allen Starr (N. Y. Med. Record, Oct. 27, 1888), the hypothesis of a "suprasensual combining centre" is pronounced superfluous. The combination of the visual and auditory impression is said to be *virtually* effected when these simultaneously exist in the brain, and hence in the unity of consciousness of the individual.

The name, moreover, constitutes an important rise above the level of sense perceptions, and marks the initiation of a process that is to lead to all abstract thought.

The second step in this process is taken when the name of a single object is generalized to others so as to form a class.

Taine tells a pretty story of his little girl to illustrate these early efforts at classification. She had learned to call a lamp "brûle," and was also in the habit of playing hide and seek with her nurse, with the exclamation "cou-cou" uttered as the nurse's head disappeared behind her apron. The first time the child saw the setting sun she exclaimed, "a brûle cou-cou!" The new object was brilliant like the lamp, and disappeared like the nurse's head. The child imitated the logic of her Aryan ancestors, in combining this new double experience into a single expression containing the two characters of each of the others.

Thus the second step in language is a process of generalization by means of observed analogies. Between individual objects a complex mental concept is formed, existing nowhere in nature, but only in the mind of a human being holding it. In the act of extending an individual name to a class, the little child passes out of the animal world into the human world; he becomes a rational being. For this reason some thinkers, as Professor Harris, have held that the possession is not only the sign of the soul, but the demonstration of its immortality. Whether this be so or not, the possession is none the less marvelous.

When verbal signs have once become associated with objects, it is possible for the mind to occupy itself

exclusively with them, and altogether to disregard the objects. It is as with signs of number, with whose aid, most complicated operations can be performed by the mathematician, which would be quite impossible if he were obliged to handle the concrete material objects to which these signs originally referred.

By means of signs, verbal or algebraic, the mind emancipates itself from things; by analogy and metaphor and combination, it contrives to clothe the suggestion of a single root, with endless successions of meanings, among which the original significance may be entirely forgotten.

Thus the fundamental fact in the acquisition of language is, that it arouses the activity of the highest centres of the brain—the ideal or concept centres without whose functions all knowledge of the external world must remain as isolated groups of sense impressions. Language is essential to all but the simplest forms of thought, because the registration in the brain of a combined impression or personal experience, derived from the union of two or more sense impressions, is always attended by such a diffusion of excitation to the speech centre, that the organs of articulation are called into play, and words are pronounced. This at least is the case while speech is being generated or acquired for the first time. Subsequently, the utterance of speech aloud may be restrained; but none the less is the speech centre thrown into activity, and the word re-echoes in the brain to the footfall of the thought.

The acquisition of foreign languages modifies the cerebral processes just described, by rendering them even more subtle and complicated.

The nervous tissue of which the brain is composed,

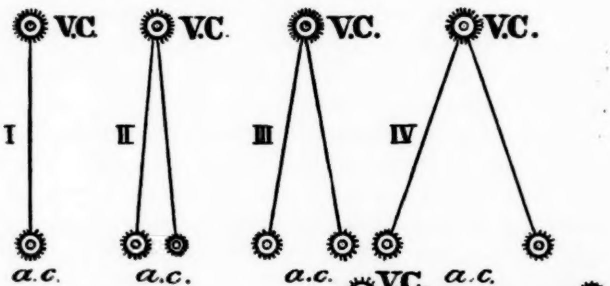
and to whose structure I have summarily alluded, is composed of an immense quantity of microscopic cells, traversed by delicate fibres, connected with each other by fine fibre-like prolongations of their own substance. By means of somewhat coarser fibres, separate territories of cells, and cells and groups of cells and fibres in the brain, become grouped together. It is because of the possibility of infinite variety in these groupings that the possibilities of speech are practically infinite.

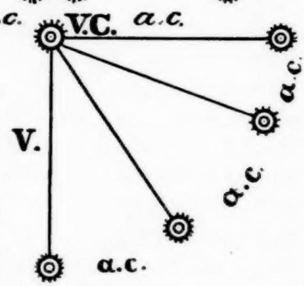
The registration of a spoken word involves, we may say, schematically or provisionally, the excitation of as many nerve cells in the auditory centre as the word is composed of separable sounds. Thus, the word "father" implies two distinct excitations, one for the sound "fa," and the other for the sound "ther." Let us suppose now that another auditory impression be made, for the same object, by its Latin name, pa-ter. This name does not only correspond to the same object; it is philologically identical with the English word, the Latin being merely a modified articulation of the same root and termination. If therefore, having pronounced the syllable fa, we then pronounce the syllable pa, we must infer that the brain of the person perceiving the difference, registers the second syllable in a different, but closely adjacent locality to that registering the first; we may suppose, in the very next nerve cell.

If an object be successively described by two names whose sounds are not identical and which are derived from different roots, then we must suppose that not only different nerve cells, but different, and perhaps rather widely separated groups of nerve cells receive the auditory impression. Thus the English and German names, man and Mann, identical with each other, are entirely different from the Latin and French

homo and homme, which are identical. The nerve territories impressed are not therefore adjacent, and when the double sets of verbal signs for the four languages become associated in consciousness with the same object, we must suppose that the impulses converging upon the visual centre, to combine with the visual impressions of the object, are gathered from a larger area than when only a single auditory sign has been used.

The area is still wider if there are four entirely different words in the four languages known. The different conditions in the four cases may be represented thus: (it must be remembered that whenever two distant regions are affected, the fibres connecting the two must also be modified.)





V. C. visual centre.
a. c. auditory centre.
I. With single name or auditory impression; *e. g.* the English name plate, without its foreign equivalent.
II. With identical root in two languages and modified termination; *e. g.* homo, homme in Latin and French.
III. With identical root sound slightly modified by articulation into equivalent sounds; *e. g.* pater and father in Latin and English.
IV. With entirely different names or root sounds for the same object in two languages; *e. g.* mensa and table in Latin and English.
V. With different root sounds for four languages.

Adopting the convenient schematic representation of the cerebral process involved, as a vibration and combination of vibrations, we may compare the successive complications to the vibrations of piano strings combined as follows:

I. Tone A combined with tone B.

II. Tone A combined with tone B and semitone C.

III. Tone A combined with tone B and tone C.

IV. Tone A combined with tone B and tone G.

V. Tone A combined with tones E, F, G, in another octave.

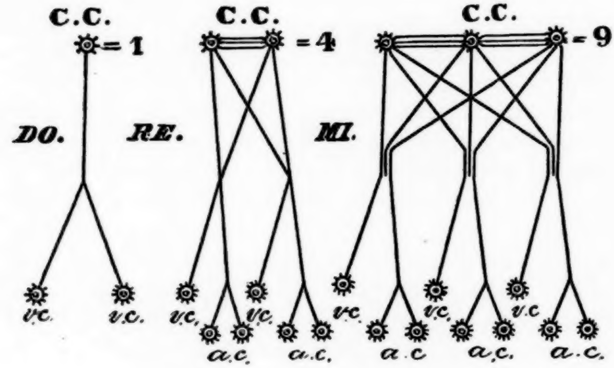
The various combinations and extensions of the area of cerebral excitation are effected even while the process remains limited to the instinctive acquisitions of multiple verbal signs, *i. e.* of two or more languages, by such unconscious effort as a child expends in learning his own or a foreign tongue in the nursery. But in the deliberate study of several languages, the complex combinations effected between the visual and auditory centres are carried up into the ideational or concept centre, there also to widen the area of excitation and increase its complexity.

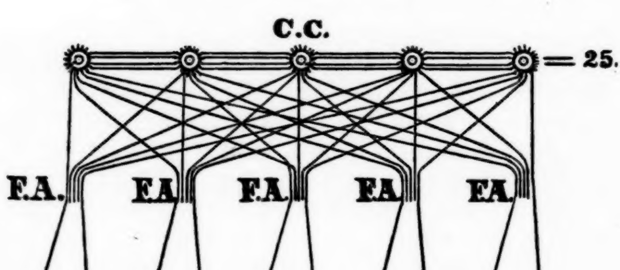
To illustrate: let us call the first combination above described, AB; the second, ABc; the third, ABC; the fourth, $ABCDEFG$; the fifth, $AB+$, representing extension to another octave, with affection of all the intermediate notes.

These combinations may then be indicated by single symbols, which, to continue the analogy with musical vibrations, we will take from the musical scale: $AB =$ Do; $ABc =$ Re; $ABC =$ Me; $ABCDEFG =$ Fa; $AB+ =$ Sol.

The more complex the vibration transmitted to the concept centres, or to those parts of the brain where

visual and auditory impressions and their combinations rise into consciousness, the more complex the excitation which will be produced at these latter centres. This is equivalent to saying that there will be a more complex and varied generation of ideal impressions or ideas, by whatever mysterious process that may be brought about.





Thus, in this diagram, let *C. C.* represent the concept centre, thrown into vibration by impressions produced during the various combinations of the excitations of the visual and auditory centres, *V. C.* and *a. c.*

The diagram is intended to illustrate in a rude and approximate way, how the more complex vibration, as represented by the symbols, affects a larger area of all the groups in the concept centres, and also how the intensity of nerve vibrations at these points must rapidly increase. The increasing intensity is represented by the figures 1, 4, 9, 25. The series could of course be indefinitely extended, yet must always fall infinitely short of the complexity of the actual process.

To sum up: the acquisition of foreign languages in addition to the native tongue multiplies the number of verbal signs which the mind habitually couples with visual impressions. In registering and in using these multiple signs, the mind is compelled to more complex operations than when only one sign is used. When in different languages different primary words or roots are used to represent the same object, then the mind, using them all, becomes acquainted with the several aspects of that object which have impressed the minds of those among whom these different names have sprung up. Thus a larger impression of the object is formed, and the mind of the speaker, which is rendered more flexible and active by engaging in more complex internal processes, is also enlarged by a richer store of external impressions. This latter effect is proportioned to the degree to which the different language aspects of the object are thoroughly studied; it may be entirely missed if they are not deliberately studied at all, but words learned only by rote or by habit.

It is finally to be noticed that while the mental or cerebral process increases in extent when multiple names are learned which have no relation to each other, *i. e.* which come from entirely different roots, the delicacy and finish of these processes is more increased by the study of closely related words, *i. e.* those with precisely the same root and only modified termination, or those whose identical roots are modified by the introduction of equivalent sounds, as p for v, g for k, etc.

The reason for this is the same as for any nervous action, and is conspicuous in nervo-muscular actions. Every one knows the immense superiority in delicacy and subtlety of the movements performed by the fingers as they pass through minute areas of space, as compared with the movements of the arms or legs, which may extend so much further. And in a similar fact lies the reason for the immense mental discipline to be derived from the study of the European languages, which are all so closely related as to be scarcely more than cognate dialects of Greek, Latin, and Gothic. The discipline is only obtainable when these languages are studied together as simple varieties of the European language. To study them separately and successively is as illogical and time-wasting as it would be to concentrate isolated attention upon peach blossoms or plums, instead of considering at once the great rose family, of which they are members. Neither in the botanical nor in the philological family can the characters of genera and species be understood without incessant reference to the more general characters of the class to which they belong. This reference is even more important for languages than for plants, on account of the incessant transformation of the one into the other, and of the historical phenomena of

development and decay which they share in common with living organisms. In the attempt to acquire an empirical acquaintance with apparently unrelated facts, enormous amounts of time are wasted, which would be saved by the scientific insight into the real relations of these facts, with which the study might just as well have been begun.

The first question we have proposed for solution may now be answered thus. There *is* a special effect produced by the acquisition of language, so special that it serves to distinguish man from the brutes. It depends upon and incessantly develops the ability to use abstract signs as symbols of things, and to use them apart from these things. It is essential to the elevation of the mind above the level of sense perceptions; and itself develops the mental sphere in which ideal conceptions arise, combine with one another, and generate endless successions of new ideas.

The process of acquiring foreign languages, in addition to the mother tongue, modifies the original process, by extending, refining, and complicating it. Impressions are immensely multiplied and the mind becomes accustomed to take cognizance of such subtle differentiations that its delicacy of perception is indefinitely increased. The capacity to appreciate subtle distinctions, more subtle than those existing in nature outside of the mind, is essential to scientific work. It is also essential to a high grade of ethical culture. Not unjustly have language studies been entitled "Humanities"; for it is the grade of mental development which they foster, that is necessary for the harmonious and finely equitable maintenance of social relations. Without this culture, the study of the external world, even if successfully pursued—which is rarely the case—is

liable to have a materializing and even brutalizing effect, and that in proportion to the complexity of the interests involved. It is very possible for an illiterate carpenter to be a very honest fellow; but it is much more difficult for an illiterate physician to be truly honorable, even when skillful in his craft.

II.

Language is not the only abstraction to which a young child becomes accustomed. The abstraction of number comes to him very early, and the study of arithmetic should even precede the systematic study of language.

Our second question demanded a comparison of these two forms of abstraction, Language and Arithmetic. The comparison is not difficult to make. Number is a single quality abstracted from objects, to be handled separately by means of its signs. But words represent multiple qualities combined in constantly varying proportions.

When the child first learns the principles of number, it must not abstract this quality from concrete objects; but these are to be handled until a number of concrete visual impressions have been firmly engraved upon the mind.

With words, however, the association with visual impressions, which is so much more complex, must also be maintained for a much longer time. For two or three years, no word should be given to the child or handled by him which cannot be directly referred to sense perceptions; and it is indefinitely desirable to revive their association and to make it as vivid as possible.

Thus, mathematical signs, earlier detached from

objects, soon pass into a more purely abstract region than words, from which the image of the object is never completely effaced, and which indeed constitute forever a marvelous transition ground between purely mental conceptions and purely sense impressions. The high degree of abstraction of mathematical signs, however, is balanced by the much greater simplicity of their mutual relations; while the more concrete and sensuous character of verbal signs is associated with an incalculable multiplicity and qualitative variety of interrelation. Hence they bring the mind much nearer to the infinite variety of nature than does mathematics. The abstractions of language prepare for the copious details of natural science and of practical life; the abstractions of mathematics, though essential to the scientific manipulation of these details, are liable, if uncorrected, to unfit the mind for their assimilation. Mathematical training facilitates the working of the syllogism; but language training tends much better to facilitate the discovery of the premises.

Let us now compare the study of language with the study of physical science.

Physical science consists of two parts: 1st, the acquisition of sense impressions through contact with external phenomena. 2d. The collation, comparison, and classification of these impressions, reasoning upon them, and establishment of the laws of phenomena.

The first process collects the raw material of science. But it is the second process that creates science out of its raw material. Science is not nature, but the product of the mind acting upon nature.

Thus the first process in scientific study corresponds to the activity of sense impressions, which for every

individual constitutes the earliest form of conscious activity. The second process corresponds to the second step, taken when the mind reacts upon its sense impressions sufficiently to generate words, to create language. Words are the first products of the action of mind upon nature, as science is the latest and most complex expression of the same action. Thus language is the earliest and most perfect type of science. In its three-fold nature it offers a three-fold type, namely, in words, in grammar, and in literature.

Words, as has been shown, result from the combined activity of several sensory centres in the brain, taken together, or further combined with that of its ideal centres, the latter being, probably, portions of the brain which are not immediately connected with sensori-motor apparatus or with sense impressions. Words may, therefore, be compared to the centaurs of antiquity which were half man and half beast. For on one side they contain the image of external objects ; on the other, they consist of a mental sign which has been generated within the brain. Hence, words may be studied in a twofold manner, objectively by methods appropriate to any study of objects, while subjectively they may be utilized to exercise the mind in handling abstractions not yet disconnected from concrete things.

Now, it is quite impossible permanently to choose, as some people seem to imagine, between study of words and study of things, after the very first steps have been taken. The first steps must certainly consist in direct observation of things, and in training the senses by such observation. This doctrine is very recently enunciated, but now commands general acceptance. We know now that the use of language does not indicate the first activity of the mind, but the second.

Education should not, therefore, begin with language, with the alphabet, and reading and writing, any more in the mother tongue than in a foreign language. It should begin with the systematic training of the sense activities that occupy the first six or seven years of life and alone are consciously exercised at this time; the growth of speech, though proceeding with marvelous rapidity, being a quite unconscious process. I have said elsewhere that a child who is taught words before he has learned to handle things is liable always to rank things in subordination to words, a dangerous and often fatal error. But in the handling and observation of things by a young child there soon comes the necessity for a pause. The necessity depends upon two circumstances ; the material to be studied is difficult of access, and its important properties are too complex and too recondite to be made appreciable to the child's senses, consequently not at all to his mind. Because a simple sense perception is possible to a child at the time that a complex mental relation would be incomprehensible, it does not follow that a complex sense perception is more easily appreciated than a simple mental relation. Still less does it follow that it is possible to convey to a child knowledge of many of the most fundamental facts of science, which are not merely phenomena of nature, but complex ideas, composed partly, indeed, of observations of phenomena, but partly also of the inferences, often very subtle, which have been based on these observations.

It is a most ludicrous misconception of the nature of science to suppose that the little manuals and primers which abound for the purpose of disseminating information apart from scientific method, really teach anything at all. Again, it is a most dangerous prep-

aration for the study of science to call upon children to imagine or represent to themselves facts which have not been apprehended by their senses, or those which could never be. Why should we try to make a child believe that the earth goes round the sun, a statement which contradicts all the experience of his senses? I should rather tell a child, if interrogated, that I have heard that some people said so, but that I myself had no real knowledge on the subject; which is strictly true. Scientific imagination is only permissible to those whose minds have once become saturated with pictures of real things from prolonged contemplation of nature. The interposition of drawings, schemes, models, diagrams, and the like does not facilitate knowledge of nature, but tends rather to fatally defer the possibility of attaining this knowledge. Hence, until the real objects can be perceived, and by means of the real scientific methods, there is nothing gained, but only precious time wasted in pretending to study them. This same precious time can, however, be utilized in the study of a class of objects which are everywhere accessible in abundance, and whose properties can be rendered conspicuous and intelligible to a properly prepared child of seven or eight. This class of objects consists of words.

There can be no antagonism between the study of things and the study of words; but the first must initiate education, and the second take it up when further progress in the first has become too difficult. To the study of words, as I propose to show, may be brought the scientific methods used in the study of things—observation, analysis, comparison, classification; and the child may thus begin to be trained for

physical science at a time when the pursuit of most physical sciences is impossible.

The purely descriptive sciences of botany and map geography, already begun, may indeed be slowly pursued; but the most strenuous study for the time should be that of language. This study does not merely serve to occupy the time and to acquire a kind of knowledge necessary for practical purposes at a time when such acquisition is most convenient. But it provides, even in its first stage—the study of words—a discipline that is quite indispensable to the pursuit even of physical science, whose alleged utility is so often contrasted with that of language.

The habit of handling abstractions, if not exactly essential to the simplest perceptions, is essential to all thought about these perceptions. It is essential also to all perceptions beyond the simplest and most obvious, for the larger part of what the mind perceives is what the mind brings to the object from its previous store of knowledge and reflection. Every word is a condensed generalization of experiences or of observations. Only those accustomed to words are successful in condensing into unity even their own observations; still less, those of multitudes of other people.

The second part of language, grammar, affords still higher training in the mental processes involved in scientific study. Grammar is the science of relation between conceptions. It is the science of propositions, of the laws whereby words so group themselves in consciousness as to form distinct complex ideas. We have supposed that individual impressions depend upon the excitation of definite areas of brain tissue, and that verbal impressions were peculiar in resulting from the combined excitation of several such areas.

A proposition implies the coincident excitation of a much larger number of areas, and especially in the non-sensory concept centres. The physical basis of the relation of the parts of speech in a sentence to each other we must represent to ourselves to be the vibration of the fibres (the associating fibres of Meynert), which connect these several excited areas and bring them into material relation with each other.

The study of grammar, therefore, differs from the study of words in two ways. It calls into play more predominantly the concept centres as compared with the sensory centres; and it emphasizes the excitation of the connecting fibres of the brain rather than that of the ganglion cell areas which they connect. Grammar, which from a certain standpoint is justly considered to be a branch of logic, disciplines the brain in handling and grouping the impressions which have been registered on it. The discipline thus obtained prepares the mind to similarly group and handle all new impressions; prepares it, therefore, to find a discipline in the material of physical science, as it could not otherwise do. Without such previous training in language, the mind is almost inevitably staggered and confused by the immense mass of impressions it tries to grasp in either physical or moral science.

Literature, the third department of language, represents the action of mind upon nature in a manner co-equal with that shown in science. To enable adolescents to become acquainted with European literature, it is necessary that in childhood the preliminary work in the lower departments of language, words and grammar, shall have already been accomplished. In words and grammar are already found outlined or reflected the history and the philosophy

of European nations. Studied with the same system and method that would be applied to the material of a physical science, words and grammar will lead the child insensibly, but profoundly, into the very heart of literature, and into the central life of the races of humanity that concern him. Until he has touched upon this, his own is incomplete.

The foregoing considerations answer, we think, the second question, which asks a comparison between the educational values of language, mathematics, and physical science.

They also answer the third question, namely, when the study of language may be most profitably pursued. The characteristic time for this study is between the age of seven, as the kindergarten training closes, and the age of fourteen or fifteen, when really scientific studies may be begun.

III.

I have asserted a little while ago that the most characteristic benefits to be derived from the study of European languges are only obtainable if several of them are studied simultaneously, and on the same plan with which we should study the different members of a single botanical family.

The table below shows the division made by modern philologists of the great Indo-European family of languages. Out of these it is sufficient, both for practical and theoretical purposes, to select three branches, the Greek, Latin and Gothic branches. From the first two we need Latin, Greek, and French. From the third, English and High German. Knowledge of these five languages is requisite to the real understanding of any one of them; and if these are possessed, knowledge of the remainder, though often most interesting, is

unessential and may be deferred or neglected. Thus as a modern representative of Latin, either French or Italian, perhaps even Spanish, might be selected; but on the whole, to-day, a practical acquaintance with French is most often required; and, as Milton observed, any one who knows Latin should be able in three weeks to learn Italian. It is hardly necessary to observe that these languages contain the literature and mirror the thought and life of Europe. Nor is it necessary to dwell on the vulgar error which would distinguish Latin and Greek as dead languages, and hence less useful than modern dialects which may possibly be spoken. To an English-speaking person of any culture, Latin and Greek are far more living than Spanish or Portuguese or Dutch, all spoken languages. Five-sevenths of our English vocabulary is Latin.[1] As Prof. Harris remarks, we are still living in the midst of Roman civilization. Yet Greek is so much nearer the complex flexibility of modern habits of thought, that Dr. Schliemann might almost be justified in urging its

INDO-EUROPEAN LANGUAGES.

ARYAN.	SOUTHWEST EUROPE.	NORTHERN EUROPE.	
		Sclavonic.	
		Bulgarian.	Bohemian.
Sanscrit.	Greek.	Polish.	Lithuanian.
	Modern Greek.	Russian.	Old Prussian.
		Teutonic.	
	Latin.	Gothic (extinct).	
Iranian.	Italian.	Scandinavian.	
	Spanish.	Danish.	
	Portuguese.	Swedish.	
	French.	Norwegian.	
Zend.		Icelandic.	
		Germanic.	
	Keltic.	Low German.	High German.
	Tribes in Spain.	Friesic, Dutch.	English, comp.
Old Persian.	Gaul.	Anglo-Saxon.	from
	Britain.	Old Saxon.	Anglo-Saxon.
Armenian.	Ireland.	Low German.	Latin.

[1] Whitney: Life and Growth of Language, p. 117.

acquisition before Latin, and as a spoken conversational tongue. It is, moreover, as is just beginning to be noticed, a really modern and still spoken language; but this consideration is practically less important than the others adduced.

With which vocabulary from among these languages a child begins his systematic study of language, is almost a matter of indifference. Still, it is usually preferable to select Latin, because its letters are the same as English, as is not the case with German and Greek; because the structure of its words and spelling is most closely allied to English, which is not so obviously the case with French, whose pronunciation also offers peculiar difficulties; and finally, because the regularity and simplicity of its grammar render it the language in which the principles of grammar should first be studied. Greek grammar is more complex; French and German, more arbitrary and capricious, especially French. English grammar is atrophied, and as unsuitable as a field wherein to learn the principles of grammar, as the hoof of a horse would be as a model for the study of feet.

It is desirable, when possible, that a child learn instinctively two languages from birth; but it is also desirable that no attempt be made to teach it to speak more than two. Supposing these two languages to be English and German. At the age of six and a half or seven, a dozen lessons should suffice to initiate the child into reading the same, when he is only obliged to translate the new visual signs into auditory signs with which he is already familiar. The initiation once effected, it is quite unnecessary to pursue further special systematic instruction in reading and writing these two mother tongues; knowledge of which will be picked up incidentally, and much faster than by the

usual methods. But the child may at once, at the age of seven, begin to read in Latin and French simultaneously. It is not customary to consider this possible, because the study of foreign languages is habitually initiated by the study of their grammar. But this is as unphilosophical as was the former practice of beginning the study of English with spelling and grammar.

Children tend to learn a foreign language by precisely the same process by which they acquire their own. They first learn words, and are so powerfully impressed by the roots of these, which convey all their essential meaning, that they remain perfectly indifferent to their collocation, termination, and inflection. If, disregarding this natural tendency, the teacher compels the child to study grammar first, an opportunity to learn a great deal is wasted, and much time is also wasted in learning a very little.

Part of the mistake depends upon the assumption that a child must be taught to speak the language before learning how to read it; and for speaking correctly, a knowledge of grammar and idiom is indispensable. This is the view taken of the modern languages. But another mistake is made when Latin is considered; for as a really fluent reading knowledge of Latin is to-day rarely aimed at, the advantage of its study is often supposed to lie exclusively in the discipline afforded by its grammar. Hence, with French, the child is tied down to endless uninteresting questions about the umbrella of my aunt and the inkstand of my grandmother, in the useless attempt to teach him to speak French correctly; or in Latin is drilled upon the galloping of swift legates from the armed city, so that he shall be able to parse Caesar's Commentaries. Yet I imagine that even Roman children did not trouble themselves much about legates. And the con-

versational methods of modern French text-books, often admirably designed when the time has really come to teach grammar, will, when premature, only serve to suggest to the child, as I heard one say, "that the French must be an awfully inquisitive people to ask so many foolish questions."

The manipulation of a foreign language by speaking and writing it to express one's own ideas is a much greater cerebral effort than is generally recognized. It is an effort that is not demanded at the same stage of knowledge about any other subject. For instance, a student is expected to spend a very long time upon the study of descriptive botany before he would be called upon to invent botanical theorems of his own. Speaking a foreign language is the mental equivalent for thinking out original propositions in a foreign science. The difficulty is usually evaded by the student using some hybrid form of speech, as Roger Ascham long ago remarked was the case with young English children compelled to speak Latin,—or rather in a barbarous gibberish that rather deferred than facilitated their acquisition of the classic speech.

It is the study of words, which corresponds to the descriptive study of the details of a science, with which the mind must become saturated before it attempts to re-arrange their relations into new formulae. It is the study of words, therefore, which should come first—*not* the attempt to use them, except where the language has been learned instinctively in the nursery.

The words cannot certainly be learned in rows out of a dictionary, but only in connection with their context.

For Latin it is well to construct simple sentences containing only a subject, object, and verb in the third person, which sentences the child must be shown how to read, translate into English, and then write out a

translation into French. This can be done at the very moment the child is still learning how to read in English, and an immense amount of time thus be saved. A three-fold impression is made upon the mind ; the words in the three closely allied languages fuse readily into a complex conception, which retains its several parts much more firmly than when each is learned separately. At this epoch the mind is naturally quickened for the acquisition of verbal signs, and the acquisition of one facilitates that of the rest.

When any set of mutually convertible sentences has been written in the three or four languages, the words in them may be picked out and their roots compared with one another. At seven years old it is quite easy for a child to learn to understand the nature of roots. In his own use of language, as has been said, a child cares for nothing else. He is very much in the condition of his primitive Aryan ancestors. Remembering the fact that to a child anything may be made intelligible which is appreciable by his senses, it is clear that there should be no difficulty in pointing out to him the affinity of the sounds produced by the same organs of articulation. He can be easily taught to distinguish gutturals, dentals, and labials, or even the distinction of surds and sonants, and thus to learn the facts at the basis of Grimm's law.[1] In the table below is shown

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[1] GRIMM'S LAW.

	Original Sound.	Sanskrit.	Greek.	Latin.	Gothic and Low German.	High German.
Aspirates	kh	gh (h)	χ	h, f (g, v)	g	k
	th	dh (h)	θ	f (d, b)	d	t
	ph	bh (h)	φ	f (b)	b	p
Sonants	g	g (i)	γ	g	k	ch
	d	d	δ	d	t	zz
	b	b	β	b	p	f, ph
Surds	k	k	κ	c, q	h, g (f)	h, g, k
	t	t	τ	t	th, d	d
	p	p	π	p	f, v	f, v

the method of analysing, for a child seven or eight years old, the Latin words *tectum, frango* and *calidus*, and their philological equivalents in French, German, and English.

[1]T [2]E [3]C [5]T [6]UM	[1] Dental letters all correspond.
[1]T [2]O [4]I [5]T [6]()	[2] Vowel " " "
[1]D [2]E [3]CK () [6]()	[3] Guttural " " " and duplicate.
[1]D [2]E [3]CK () [6]EN	[4] Guttural broken down into i.
	[5] Dental letters correspond.
	[6] Terminations omitted for *toit* and *deck*.

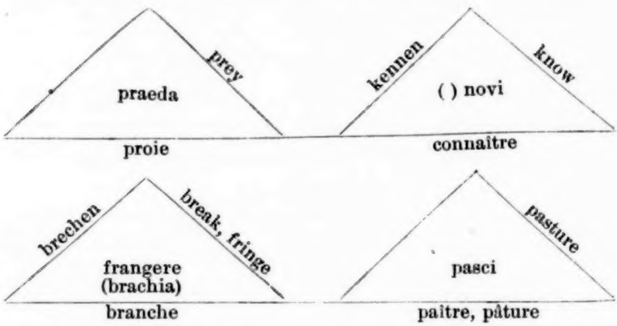
[1]F [2]R [3]A [4]N [6]G [7]O	[1] Labial letters all correspond.
[1]F [2]R [3]I [4]N [6]G [7]E	[2] Liquid " " "
[1]B [2]R [3]EA [4]() [6]K [7]()	[3] Vowel " . " " or duplicate.
[1]F [2]R [3]A [4]() [6]G [7]I [7]LE	[4] Liquid " " " ; omitted for
[1]B [2]R [3]E [4]() [6]CH [7]E [7]N	*break, fragile* and *brechen*.
	[6] Guttural letters correspond.
	[7] Terminations omitted for *break*.

[1]C [2]A [3]L [4]I [5]D [6]U S	[1] Gutturals correspond.
[1]CH [2]A [3]U [4]() [5]D [6]()	[2] Vowels correspond.
[1]H [2]EA [3]() [4]() [5]T [6]()	[3] Liquid broken down into u, or omitted.
[1]H [2]EI [3]() [4]() [5]SS [6]()	[4] Vowel omitted in others.
	[5] Dentals correspond.
	[6] Termination omitted for *chaud, heat* and *heiss*.

Thence the step is easy to the recognition of the equivalent letters in the corresponding words of different European languages. Practically, he thus learns half a dozen words in the time usually occupied in learning one; learns them with six time the vividness, and is six times less likely to forget them. Philosophically is laid the basis for the conception of central unities diversified by superficial differences, which is one of the fundamental conceptions of both philosophy and science.

In the *Teacher* for June, 1888, I have described a little device for the comparison of verbal roots, which I have called "language triangles." The child draws one side of a triangle; in the centre of this he writes the Latin word. Under the base line, and thus parallel to the Latin, he writes the French word, when it really

has the same root; and on the two other sides, opposing each other, he writes the German and English. The words so taken must always have the same root. If in either of the four languages the object or idea is expressed by a different root, the space for that language is left blank on the triangle.[1] In addition to those published in the *Teacher*, I subjoin some others.



At a later period of study, and when enough grammar has been acquired to render possible the correct construction of sentences, the comparative study of words may be pursued in a somewhat different fashion. A Latin word may be selected from a sentence in which it has been read, and introduced into a new sentence, to be devised by the child. Then sentences in English, French and German, later in Greek, must be similarly devised so as to introduce the cognate meanings of the same root, with their expressive shades of difference. The system simply extends the method, already in current use, for studying English synonyms. A few examples may serve to illustrate the advantage of this device.

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[1] See *Teacher* for June, 1888. New York.

Multas virorum clarorum statuas in templo *posuit* dux.
Je sais *positivement* qu'il va pleuvoir.
We *postponed* going to the picnic.

Oppidum obsessum *victus* satis habuit.
There was an old woman, and what do you think,
She lived upon nothing but *victuals* and drink.

Puella regi se *tradidit.*
The boys *traded* marbles with the girls.
Le *traître* a *trahi* le roi.

By such methods, the child, at the end of a year, may be expected to learn from three hundred to five hundred Latin words, and their cognates in French, English, and German. It is not to be presumed that these will be learned beyond the possibility of forgetfulness; but at least the first impression of them will have been made upon the brain. At the same time a beginning can have been made in reading French, whose resemblance of construction (though not of verbal structure) to English makes it easier to read than Latin. A book interesting to the child must be selected, and the reading conducted exactly as it would be in English: the words spelled by the sound of the syllables, the meaning of each word told, and thus the phrase slowly interpreted. When the interpretation is once complete, the child must read the phrase over and over again, until it can be both understood and enunciated fluently; for until the fluency is attained—and it always can be by sufficient repetition—the phrase does not represent language to the child. The attainment of fluency in the reproduction of the impressions made by the written phrase is not only important in itself, but it serves as a type of complete knowledge in any subject. Any kind of knowledge is only thoroughly grasped and digested

when all parts of the brain tissue impressed by it vibrate easily and harmoniously upon its suggestion. In reading words, so long as there is stumbling in enunciation, the knowledge has not risen to the rank of an acquired language, the isolated words have not fused into expressive speech.

It is to facilitate fluency, that for at least two years the teacher must supply the place of the dictionary, and tell the child the meaning of each new word. At most, when a word once learned has been forgotten, he may be led to refer to the previous phrase, and to recall the meaning from the context. It is indeed always desirable that the meaning of new words be divined as much as possible from the context. It is wrong to condemn this as reprehensible "guessing" and fatal to accuracy and thoroughness. The act implies an effort of the mind to revive faint reminiscences and to detect faint associations of ideas; it is the very act involved in scientific research after new truths. The teacher must of course be on hand to test the accuracy of the guess, and correct it if wrong or flippant.

The child should not at this period be left to himself at all. Argument as to what is intellectually possible for a child, must not assume that he is to be thrown upon his own resources to interpret the French or Latin page. Such independent work comes later. But the business at first is *not* to train the mind in self-reliance, but to saturate the brain with impressions, and to habituate the ear to a new form of speech. This must be done under guidance, as clay is moulded by the guidance of the sculptor's hand.

With such guidance, Latin—its construction somewhat modified—may be read with but little more diffi-

culty than the French or German. It is essential that the child become accustomed from the beginning to at least the easier peculiarities of Latin construction. It is pitiful to see scholars, after many months, even two or three years of study, still stumbling over Latin sentences, in the attempt to read them in the English order, to turn them, as it is said, "into good English." Now in order to penetrate fully into the spirit of a language, it is necessary at the moment of enunciating it, to banish all recollection of any other language from the mind. It must *not* be translated, or the habit of translation must cease as soon as possible. Then only is it evident that the mind places its different groups of verbal signs on the same footing. One great value to be derived from a fluent acquaintance with Latin and Greek is, that in passing into the unfamiliar construction, the mind passes into a separate consciousness; and by so much enlarges the range of its own experience. This cannot be done to the same extent by means of French, German, or Italian language, because their construction too closely resembles our own.

After two years' study of words, and when by repeated practice, some empirical reading knowledge of French and Latin has been obtained, the child may enter upon the second part of language, the study of *grammar*.

The fundamental peculiarity of grammar has been pointed out. It is concerned with the relations of words and ideas, quite separated from their sensory origins; concerned with processes that take place exclusively in the concept centres of the brain. These are called into function to a much greater extent than is the case even in regard to words, which indeed transcend

sense impressions, but not to the extent to which the conception of verbal relations does.

It is perfectly absurd to make a child study grammar until its mind has been well stored with impressions of words. And, on the other hand, it is equally absurd, and a great waste both of time and of fitting opportunity, to defer the study of words until the mind has become ripe for the study of grammar.

Why French grammar should ever be learned before Latin, I have never been able to understand, yet I know it is often done. A large part of its subject matter consists of idioms and conventions, whose reason lies in the historical development of the language, and not in logic. Now a child is capable of logic, long before it is really capable of history. To teach French grammar before Latin, is to accustom the child to place accident before necessity, and convention before truth; a most fatal habit of mind. The two grammars should be studied simultaneously.

In considering grammatical inflections, the child learns to develop an idea whose germ had been previously acquired, namely, that the essence of the word lies in its root, and that the termination is a varying modality. But to fully appreciate this fact, the child must be led to discover the inflections and their groupings for himself, and not confronted at the outset with lists of declensions and conjugations to be learned by heart. This universal practice is, from a psychological point of view, simply barbarous. By a scientific method the child should be led to deduce the inflections from his own observations of the facts of the text. Reading the same word in many different connections, and being obliged, by the context, to translate it differently each time, the child can be led to notice

the different termination which corresponds to each translation. From these various observations he can gradually build up for himself, of course under guidance, a complete scheme of the five declensions. Much more time is thus consumed than in the ordinary method of learning these declensions by heart. But, on the other hand, the child repeats the process by which the grammar was originally constructed, and what is still more important, he becomes acquainted with the method which is typical for all scientific study; he collates scattered facts, brings them together, observes their relations, and establishes their law.

The same method applies to the more difficult study of the verb. But here three degrees of generalization are to be observed, that of person, of tense, and of mood. The first distinction is the most general and the most easily appreciated. The extreme regularity of the person terminations in the Latin verb makes them an easy subject for drill. After they have been discovered and established, the distinctions of tense may be similarly dealt with; first, in their broad distinctions of past, present, or future time; later, in the subdivisions of past and future time, that for a long time must seem to the child unnecessarily subtle. Even more subtle are the modifications of assertion implied in moods. I do not think the distinction of indicative, subjunctive, and infinitive mood can really be made intelligible to a child under eleven or twelve years of age, if it can then. But, nevertheless, these moods can be studied descriptively at eight or nine, when they are not explained, but merely characterized by the English auxiliary words used with them, *may*, *might*, *to*, etc.

The inflections of nouns and verbs furnish the child

with conceptions of scientific classification at a time when, as already pointed out, these cannot be obtained from physical science. They furnish types of more abstract classification than is afforded by study of word roots, for inflections represent modifications of roots corresponding to modifications of the mind perceiving them. The mind does more than perceive—it handles these roots; it freely manipulates for its own purposes what has hitherto been presented to the child in a purely objective aspect. When a child learns a language on its subjective side first—learns by habit to speak it and use it as a tool, he loses the immense impression obtainable when words have first been studied objectively, as classes of things having a real and independent existence; and the mind is afterwards seen to establish a free dominion over these same things, moulding them to its own purposes, yet leaving their essential nature undisturbed. Here is a splendid type of the action of the human mind in nature, whose details once conquered, may also be inflected to express human meanings.

When the inflections have once been learned, the child must change his mode of reading. He must no longer be told the meaning of words, nor allowed to divine either the root meaning or the mode from the context, but he must infer the precise interpretation of each word and of the entire sentence from these inflected terminations.

It is generally recognized that this act of inference or reasoning is an important mental exercise. Indeed, teachers are rather liable to err on the side of thinking that this is the only kind of mental discipline, and that it is, moreover, the chief value of learning Latin. Neither assertion is true, but the value of the infer-

ence is nevertheless great. In it the fact observed, as for instance the termination of the genitive case, is first associated with previous impressions of other similar terminations and the similarity recognized. Then associated circumstances of these previous impressions are revived in memory, as the fact that the termination belongs to such a declension, and is translated by the word "of" in English. The association of these circumstances is transferred to the new impression which has been placed in the same class, and the word therefore interpreted as the others had been.

Acts of inference always imply a similar revival of past impressions, principal and accessory, and their fusion with the impression newly received. They powerfully exercise the mind because they fuse scattered excitations or vibrations into energetic unity. The inferences demanded of the child in translating Latin are simply the type of mental acts that are to be demanded of him all his life, and constitute an excellent preparation for these. The logical value of French and German is so much less, because precise knowledge of construction and inflection is unnecessary to the interpretation, and the general similarity to English renders much narrower the space traversed by the mind to reach the point of view of the foreign consciousness.

All grammatical subjects must be studied on the principles laid down for study of the inflections. The laws, as far as possible, must be deduced from observation of the facts, and not announced categorically, with the facts adduced in illustration. Grammar must be carefully kept subordinate to language considered as a means of expression and communication.

I often think that the feeling for Latin literature is

as much injured by excessive drill in parsing, as the literary appreciation of Milton was impaired by the old-fashioned drill in *Paradise Lost*. The study of grammar as a complete and highly abstract science properly belongs only to ripe minds—at earliest, to the period of adolescence.

For children under fifteen, only just so much grammar should be required as is essential to the accurate interpretation of what is read, and to the power of approximate accuracy in writing.

For young children, the selection of grammatical subjects in the order of their real comprehensibility to the growing mind is a delicate, but most interesting task. Two principles should guide the selection. First, that ideas are easy for the child in the degree to which they approach or involve sense perceptions or concrete conceptions, and are difficult according as they recede from these and become generalized. And second, that grammatical laws and rules are impressive in proportion as they seem necessary; and unimpressive, therefore difficult to remember, according as they relate to what seems unimportant, that is, to whatever is unessential to the interpretation of the sentence. Hence the parts of speech which modify the noun and verb are much more difficult to learn about than the noun and verb themselves; and it is illogical to place the study of the adjectives, and especially the study of their comparisons, before the study of the verb. Similarly for adverbs, conjunctions and prepositions, and for all devices for linking words together, and for which the child does not feel the necessity. Similarly for the relations of the parts of a sentence to each other, the discussion of subject and object, the management of the infinitive mood in its relations to the

moods of other verbs and to the accusative case; similarly with a host of other subjects that will readily suggest themselves to an experienced teacher, if examined by the test of the principles above stated. A child can become cognizant of a great many grammatical facts at an epoch when it would only be bewildered by the abstract law of these facts. It is easy to learn the fact that if a person or thing is said to be doing anything, the name of this person is put in the nominative case; and this may be intelligible in Latin, when it is quite unintelligible in English. But, at the very same time, the child may be utterly bewildered by the statement, "The subject of the verb is in the nominative case."

Again, it is easy to explain the relations of the subject to an active verb, when it is still very difficult to explain the passive verb or voice. I have noticed that children have the strongest tendency to put the subject of the passive voice in the accusative, because they declare (and with logic) that "something is being *done* to the person." And I think it is hopeless to demonstrate that the terrible verb "to be" is a verb at all. The fact can only be learned empirically, and all explanation of it sedulously avoided. The child confounds this verb with an adjective, and in doing so, merely reverts instinctively to the fundamental conception of the predicate, out of which the verb and adjective have diversely sprung. On the other hand, the picturesque expression of "strong verbs," applied to the famous eleven irregular verbs in Latin, can be easily appreciated by the child, as indicating words worn into irregularities by constant use.

In all study of grammar under the age of twelve this rule should dominate: let nothing be learned but

what is essential to the interpretation and manipulation of the language, and defer philosophical grammar to a ripe stage of mental development. The energy often wasted upon premature study of grammar is much more profitably occupied in acquiring fluency in language.

The slow, deliberate, and thorough accumulation and manipulation of verbal impressions enriches the brain. But it is the rapid and instinctive manipulation of such impressions that renders the mind agile and flexible, because it accustoms the brain to the rapid and multiple propagation of excitations, and their varied combinations into secondary excitations.

So far nothing has been said about learning Greek. I think that this should be begun gradually, between the ages of ten and twelve, at first merely by learning proper names and the words cognate to the Latin roots, as these are successively studied. By the age of twelve, a sufficient fluency in the capacity of reading and writing French should have been acquired to justify dropping its study for a while, and substituting the systematic study of Greek, this to be pursued most strenuously during the next four years.

The general construction of a language exhibits on a still larger scale than does its elementary grammar, a process of cerebral synthesis in which the "association" fibres of the brain are involved, those namely which connect separate convolutions with each other. Every special form of language construction depends upon a special grouping, not merely of different areas of cells, but of different convolutions, of distinct territories often widely separated. We may compare these different regions to groups of battery cells, standing on different tables in a laboratory, and labelled A, B, C, D,

etc. These groups may be brought into a circuit in various ways according to the order of their connection with one another. Thus we may have,

A + B + C + D, or
A + C + B + D, or
A + D + C + B, or
B + C + D + A, etc.,

the variety depending on the laws of permutation.

The different permutations correspond to the different modes in which separate brain regions may be brought into connection with each other, in the general synthesis of cerebral activity that effects the expression of speech according to the construction of a special language.

When a person, habituated to one form of construction, learns to understand fluently, to think, and still more to speak under another form, the functional grouping of these brain regions must be changed. Though the anatomical architecture of the brain remain the same, its functional relationships are rendered different. This change, like all changes for nervous tissues, constitutes an immense stimulus and excitation, proportioned to the extent of the change. To consciousness, the mind seems to have traversed a certain space to place itself at the new point of view. The physical basis of this consciousness is the space occupied by the nerve fibres of the brain, which propagate vibrations from one convolution to another. When an English-speaking person projects his consciousness into the form of language construction peculiar either to Latin or Greek, he seems to traverse a much wider space than if he simply pass from English to French, or even to German. The re-arrangement of direction for the intra-cerebral propagation of vibrations or excita-

tions, must therefore be much more extensive for the ancient languages than for the modern. Hence the mental development, or cerebral stimulus derived, must be much greater.

The special values of the study of Latin over the modern languages may now, in answer to our question, be categorically stated.

1. No European language, and no European history or philosophy, apart perhaps from the Sclavonic and Scandinavian groups, can be understood without knowledge of Latin.

2. Least of all can English language, philosophy, or history be understood, since the language is simply a combination of Anglo-Saxon and Latin, in which Latin considerably predominates, and Rome is indelibly impressed upon English history, thought, and institutions.

3. In the study of words, which should initiate the child into the study of language, the Latin roots are best fitted for beginning, on account of their familiarity, conspicuousness, simplicity, and ready manipulation.

4. The Latin grammar is the most perfect grammar of Europe, and should alone be used to teach grammatical principles, selected in the order of their natural comprehensibility for the developing mind.

5. The construction of the Latin language as a whole compels the translation of the modern mind into a form of consciousness sufficiently remote from its own to necessitate a great change in the general synthesis of cerebral activity. The same is true of Greek. The change constitutes a powerful mental exercise and brain stimulant.

To obtain the full value of the study of Latin and Greek upon the development of the brain, must be

applied the principles that are now generally, though half consciously, invoked in the acquisition of the mother tongue and of modern languages, namely, the synthetical impressions of the language as a whole must be copiously stamped on the brain before the pupil is called upon to analyze the language.

This is to be done by means of *much* and *rapid* reading. Roger Ascham tells us that Queen Elizabeth became a good Greek scholar by every year reading entirely through the works of Demosthenes and of Isocrates. The reading must be on a subject interesting to the child; hence it is scarcely possible that it be directed to classical authors usually chosen for a school curriculum. It is the fashion among some teachers to denounce "readers of manufactured Latin," and declare "that the sooner a boy can draw his Latin from the living spring of a classic author, the better."[1] This principle may or not be correct from the point of view of the Latin scholar, but from the standpoint of the physiologist and psychologist it is certainly absurd. We do not forbid English children to read English until they are capable of understanding Milton; or French children from reading French so long as they fail to understand Jomini's Art of War. It seems improbable that Roman children were ever schooled upon Caesar's Commentaries. It would be a poor commentary upon the results of the Latin scholarship of so many centuries, to assert that there are now no scholars capable of writing Latin in a way that should gradually initiate young children into the difficulties of its construction, while accustoming them to look upon Latin as upon any other languages,

[1] Six Weeks' Preparation for Reading Caesar. Note to teachers on first page.

as a medium for communicating interesting ideas, and not merely as gymnastic exercise for the intellect, concerned with ideas to which the child must be indifferent.

An immense number of Latin idoms can become familiar to a child in the same way as French idioms do, by the process of repeated observation of them in the course of reading, and this at a time when the abstract, the scientific statement, or law of those idioms could not really be grasped. Familiarity with the fact should logically precede analysis of the fact. Reversing this process, as is usually done, may make grammarians ; but, unless the study is prolonged many more years than is usually practicable, it does not enable the student to read the language. It is very rare to find that a boy or girl who has begun to study Latin at twelve can read Latin fluently at sixteen, though far more time is given to the study in these four years than should be the case, for they are too precious and too much needed for other things. If during the four years preceding twelve, familiarity with the phenomena of Latin had been acquired by frequent repetition, the subsequent scientific analysis of these phenomena, *i. e.* the grammatical study of the language, would be ten times as fruitful of result.

The development of our subject has insensibly furnished the answer to another of the questions started at the beginning of this essay. It is necessary to maintain a just proportion between the study of languages and the other studies of a general curriculum. The effect on mental development and training is to be obtained, if at all, by the age of fourteen, fifteen or sixteen. By this time the pupil requires the broader

and more robust discipline of other knowledge, pursued with the thoroughness of scientific method which will then be practicable. It is undesirable to continue the systematic study of languages at this time; they should be dropped altogether, although the habit of reading in all may be most profitably kept up, and other subjects, especially history, studied through their medium.

All that has been here said on the physiological value of the study of language applies to the developing mind—to the stage of development at which signs are being coupled with things, and the "mental couple" raised to the concept centre, and accepted as a unity in consciousness. For the adult mind, accustomed to the use of signs, the acquisition of a foreign language can have no such educational significance. It is true that an adult who has had no training in language, finds such difficulty in undertaking the study of anything else, that he is best advised to acquire a language, especially some knowledge of Latin, before attempting any other study, especially that of medicine. But he cannot derive the same relative benefit from learning the language then as if he had learned it as a child. Moreover, in learning the language, the time is relatively wasted that might be more appropriately spent in learning to grasp larger and more complex groups of facts and ideas than are presented in any but the really philological study of language.

Hence, one great reason for teaching children a reading acquaintance with four or five languages between the ages of eight and fourteen, is that by the latter age they may really know these languages, and then begin to study something else more difficult, or of more immediate practical utility.

Nevertheless, some study of language must always accompany all other studies. Language which alone perfectly expresses all internal thought, also mirrors all external things as they have ever impressed the mind of man. Language, speech, is thus truly the Logos, the intermediary between the soul and the world. It is at once the thought made flesh and flesh sublimated into thought.

But advanced philological study should be regarded as distinctly a specialty, as is the advanced study of philosophy, or of chemistry, or physics, or physiology, or any other science. That a youth must have, or pretend to have, a perfect knowledge of Latin and Greek before he attempt to acquire even a smattering acquaintance with the world around him, is certainly a traditional superstition. But by the method of language study which has been here advocated, the student may really experience the discipline conferred by language training, may enjoy the immense practical advantage of admission to all European literatures, and yet secure time for a correlatively liberal education in other directions, equally important.

PSYCHOLOGICAL LITERATURE.

I.—THE NERVOUS SYSTEM.

Ueber Entwickelung des Hirnmantels in der Thierreihe. Dr. EDINGER. XIII Wanderversammlung südwestdeutscher Neurologen und Irrenäzte zu Freiburg i. Br., Juni, 1888.

Abstract of proceedings in Neurolog. Centralbl. 1888, No. 14, by Dr. L. Laquer. Dr. Edinger showed preparations illustrating the development of the forebrain in the animal series. The brain mantel only gradually reaches the high development which it attains in the mammals, but there is not an unbroken series from the lowest to the higher forms. The purely epithelial brain mantel of the bony fish, the cyclostomes, and selachians, was demonstrated. Between these and the simple amphibian brain there are no intermediate forms. The fundamental form of the amphibian brain is to be found among the reptiles, but among the reptiles there appears, with the beginnings of the cortex, the earliest form of the brain from which that of the birds and mammals has been developed. In reptiles first appears the Ammonsformation and the associated Fornix. While the mantel undergoes all these changes, the position and structure of the ganglia of the trunk remain in general the same through the entire series, decreasing, however, in relative importance with the increase in the mantel. Commissural fibres and fibres connecting parts of the forebrain with other regions, are found in all cases.

Untersuchungen über die vergleichende Anatomie des Gehirns. 1. *Das Vorderhirn.* L. EDINGER. (Abh. d. Senckenbergischen naturf. Gesellsch. 1888, p. 91 bis 119, 4 Tafeln.) Abstracted by Obersteiner in the Centralbl. f. Physiol. No. 12, 1888.

In the bony fish the brain mantle covers the basal ganglia in such a manner as to be usually overlooked. A cortex with nerve cells is wanting in all fish and amphibia, and in the reptiles the first form of the cortex with ganglia appears. In the reptiles, too, appear the first fibres of a corona radiata. In birds the basal ganglia are developed to an extent not found in any other group, the cortex remaining but little developed and first reaching its full significance in the mammals. From the basal ganglia (the nucleus caudatus and putamen) arises the basal frontal tract (basale Vorderhirnbündel) which runs in part to the optic thalamus, and in part to portions further caudad.

Anatomy of the Brain and Spinal Cord. J. RYLAND WHITAKER. Edinburgh: E. and S. Livingstone, 1887. 8vo, pp. 135.

The title of a book like the one in question does not at the present day give a clear notion of what it may contain. Some years back,

the gross anatomy was almost exclusively meant by the term anatomy of the central nervous system, and to this gross anatomy there was now and then added a little on the tracts in the cord, and the description of one or two frontal sections of the brain and cord. Beyond this, the descriptions applied mainly to the surface of the organs, though not uncommonly something on the development of the brain was appended. Such a presentation of the subject answered the purpose very well when the anatomy of the central nervous system was but little developed. To-day, however, it has left this earlier condition far behind. The student wants to know, and must know, the finer anatomy of these organs, and the gross anatomy should be presented only in so far as the parts described and the names given are found to be really significant in the light of existing facts. For example, a clear idea of the arrangement of the parts about the lateral ventricles and the interbrain cannot be gotten unless the development of the brain is most carefully considered, and the changes from the primary to the secondary conditions are traced in ample detail. It is on such a knowledge only that a good understanding of the finer anatomy of this region can rest, and the same is essentially true for all the other regions of the nervous centres. Supposing these views to be correct, then a modern discussion of the anatomy of the central nervous system should contain somewhere in it a careful account of the embryology of the brain and cord, as a necessary corner-stone.

Whitaker's book does not recognize this aspect of the case, for its discussion of the embryology is very casual, and it goes along as though there was very little outside of its covers, although the allusions to the finer anatomy are scanty and often antiquated. Looked at in another way, however, it is a handy volume, containing rather more than one gets in the brain and cord chapters in the anatomical text-books, and the order of presentation is good. There are numerous plates, some of them original, the one showing the distribution of the tracts of the cord being specially useful. A very good feature, too, is the tabular arrangement, showing in a general way the representation of the parts seen in one cross-section in the section at another level. As may therefore be seen, the book will be useful where the gross anatomy of the brain and cord is to be studied, but for purposes beyond this its value is limited.

Annual of the Universal Medical Sciences. Edited by C. E. Sajous, M. D., and seventy associate editors. Issue of 1888, 5 volumes. F. A. Davis, Philadelphia and London.

An annual review of the progress of the medical sciences that fills five volumes, more than 2500 pages, and is liberally illustrated, certainly calls for remark. According to the preface, the interest of the Annual is in clinical data, and it is designed to be specially useful to the medical practitioner. Since through the chief editor it is intimately connected with the Jefferson Medical College in Philadelphia, one is not surprised to see that a large number of the articles are from men residing in that city, more than half the number of associate editors being Philadelphians. The articles are grouped under some seventy heads. They are not arranged always in the order which might be anticipated, but this is explained by the very limited time in which the work was put through the press. The work is more than a year-book in its plan, for many of the

articles are really short treatises, with a few references to the literature of 1887 put in at the appropriate points of the discussion. This gives it the value, in many cases, of a reference handbook. Such a work must of necessity be selective. It cannot review everything, and in this case we find about 2000 titles are referred to, the majority of these being for the year 1887, although they are not all within that limit.

The index of authors for 1887 as it appears in the Index Medicus includes something over 12,000 names. It is plain, therefore, that but a fraction of the literature is discussed, but at the same time it is only fair to suppose that the fraction in question has been selected as being that of greatest value.

One noticeable feature is the large corps of correspondents situated in all parts of the world, many of them in remote regions, who are expected to report on medical matters in their neighborhood. Such a plan, if carried out, must naturally lead to the accumulation of valuable clinical data.

In connection with the Annual there is also published, under the same auspices, a small quarterly journal bearing the title of The Satellite [of the Annual, etc.], the aim of which is to review the most important articles in the medical press at large. The first number of this journal appeared more than half a year before the Annual itself.

The impression that these two associated publications leave is not one of satisfaction. We are not speaking now of the individual communications, but of the book as a whole, indicating as it must a tendency.

For those who are exceedingly busy with such occupations as do not permit much time for reading, or for those who are remote from libraries, the Annual fills a gap, and as the numbers in both these classes are numerous, it may expect to be well received. The student and investigator will probably continue, on the other hand, to use the Index Medicus for the literature, the Centralblätter for the analysis of the current literature, and the various special year-books in which the articles reported are separately analysed, for getting a general view of advances made in former years.

The best articles in the Annual cannot fail to be both instructive and interesting to the reader, as giving the connected views of good authorities, but they furnish him in their present form with very little first-hand material on which he can form judgments for himself. This point of view would be admitted, we are sure, by no one more readily than by the writers of the articles themselves. In our opinion, then, the Annual is but a slight addition to the force of working books on which the student depends. It is with interest that one awaits the developement of the issue for 1889, for so much of the present volumes is necessarily standard matter elaborated in order to show the precise relations of the advances of the year, that it is not easy to see how it can be repeated in a subsequent issue without losing freshness, and if it is not repeated in some measure, the narrative style in which the articles are now written will be maintained only with difficulty.

Turning now to some of the articles which relate to the nervous system, the first volume opens with the discussion of "The Diseases of the Brain and Spinal Cord," by Dr. E. C. Seguin. In treating here of the advances made in the localization of function in the human brain, the author thinks that comparative physiology can

claim but very little credit for the present results, and that the same method which led Broca and Hughlings Jackson to locate their respective centres would have given us all the information which we now possess about centres, quite independent of the fact whether animals were or were not studied. That the details of the human brain are to be made out from the study of the human brain alone is a point that will probably be admitted on all hands; but that we should be where we are now in cerebral anatomy and surgery, without the study of the brain in lower forms, is by no means to be readily admitted when we compare the advances in the period which elapsed between Broca's observation and the fundamental experiment of Fritsch and Hitzig, with those in the same number of years following the latter.

The second volume opens with an account of the surgery of the brain and nerves, by Dr. N. Senn, in which the modern surgical methods as applied to the brain are detailed.

In the fifth volume Dr. Spitzka gives fifteen pages to the anatomy of the brain. A number of the papers which form the basis of this article have been given in abstract in this journal during the past year, but others have not been mentioned. In discussing the paper of Adamkiewicz on the circulation in the ganglion cell, and the observation of Fritsch that blood-vessels are found within the protoplasm of the giant cells in Lophius piscatorius, Spitzka seems a little hasty in saying that observations have been made which entirely dispose of the old view that the ganglionic element is the equivalent of a simple cell. In the first place, the observations of Fritsch have little in common with those of Adamkiewicz, and it is hardly fair to class the two together; and in the second place, if our notion of the simple cell is to be disturbed by finding some other structure in its protoplasm, it should long ago have been upset by finding intracellular nerve terminations and the nephridia, etc., which are intracellular in the invertebrates. Most interesting are Spitzka's own observations on the cetacean brain, in which all the parts connected with the auditory nerve are found in such a hypertrophic state on so comparatively simple a background, that it affords not only much evidence in favor of views derived from the study of animals less suited to show these points, but stands as one of the most striking contributions of the comparative method to the finer anatomy of the brain.

At the meeting of the Congress of American Physicians and Surgeons held at Washington, September 18–20, 1888, neurological matters had much attention even outside of the discussions in several societies. On the evening of September 19th there was a general discussion of localization in the brain with special reference to brain surgery. The papers of the evening were unusually satisfactory, but any special mention of them must be omitted here, because of their rather practical character. The general sentiment appeared to be that surgical interference with the brain was now attended with comparatively so little danger from the side of the operation that there might be too much surgery, and the more conservative speakers added a word of caution on this head. The remarks of Mr. Victor Horsley on an investigation which he had made in connection with Mr. Gotch, on the stimulation of the cerebral cortex in monkeys, was an experimental contribution which can perhaps be summarized.

In an attack of Jacksonian epilepsy there is first a tonic followed by a number of clonic spasms. It is known that the initial discharge takes place from the cortex, and therefore the tonic spasm is of cortical origin, but the question still remained as to the origin of the clonic spasms. These might arise either by separate discharges from the cortex, or by a rhythmical discharge of the spinal centres consequent upon a single stimulus from the cortex. Relying on the fact that each separate impulse as it passes along a nerve gives rise to a negative variation of the resting nerve current, then, if it were possible to tap the pyramidal tract above the spinal centre while the cortex was being stimulated, and examine the negative variations, it would be seen whether a series of impulses were coming from the cortex at the proper rate to account for the clonic muscular contractions. In the first place, the authors succeeded in so operating on the spinal cord in the monkey that it could be kept alive and suitable for study for nearly half an hour. With non-polarizable electrodes, they then led off the resting nerve current from the cut and longitudinal surfaces of the pyramidal tract to a capillary electrometer—a capillary tube containing a column of mercury, the height of which varies with slight variations in the electrical tension. So delicate is this instrument that it responds quite satisfactorily to variation in the nerve current by a change in the position of the mercury in the tube. The amount of this change is magnified by viewing it through a microscope. In these experiments the oscillations of the mercury were recorded photographically.

Stimulation of the cortex in the leg area gave rise to a prolonged negative variation corresponding to the tonic period; then, on the removal of the electrodes from the cortex, there followed a series of variations corresponding perfectly to the clonic period of the muscular disturbance. The origin of the clonic contractions is therefore cortical. That the result is not due to a diffused disturbance in the cord is shown by the fact that if the electrometer be undisturbed and the arm centre in the cortex be stimulated, there is no evidence of any electrical variation. An attempt to tap the motor nerve roots and test the negative variations there led to no results, the disturbance being too slight to affect the electrometer. The results in this case are highly interesting, but hardly less interesting are the several very refined methods of operation and observation by which these results were obtained.

A Comparison of the Latency Periods of the Ocular Muscles on Excitation of the Frontal and Occipito-temporal Regions of the Brain. E. A. SCHAEFER. Received February 13, 1888. Proc. Roy. Soc. Vol. 43.

The very condensed statement which the author makes of these experiments cannot be further abstracted without some important omissions. We give, however, his main points. The conjugate deviation of the eyes to the opposite side is produced by the excitation of entirely different regions of the cerebral cortex. The parts which, when electrically excited, produce this movement, are: 1, an area included in the motor or psychomotor zone of the authors; 2, the sup. temporal gyrus; 3, the upper end of the middle temporal gyrus; 4, the post. limb of the angular gyrus; 5, the whole cortex of the occipital lobe, including its mesial and under surfaces; 6, the quadrate lobule. Of these parts, the frontal area is distinguished

by the fact that its excision causes paralysis of the movement. From this fact Ferrier concluded that in the case of the frontal area the excitation was direct, while in all the other cases it was indirect, *i. e.* through subjective sensations. To test this, S. examined the latent period of stimulation of the ocular muscles when excited through the various regions named, and found that it was some hundredths of a second less in the case of the frontal area than for any of the others, thus indicating that in the case of the latter the impulses must pass through at least one more nerve centre than in the case of the former. It was most natural to infer that this other nerve centre would be the frontal area. But that this is not so is indicated by the fact that complete excision of the frontal area on both sides does not abolish the reaction when caused by stimulation of the other portions of the cortex. What the other centre may be is therefore still left doubtful. The work was done on monkeys.

On Electrical Excitation of the Occipital Lobe and Adjacent Parts of the Monkey's Brain. E. A. Schäfer. Rec'd Feb. 13, 1888. Proc. Roy. Soc. Vol. 43.

By stimulating the parts of the cortex named, S. has found that not only were movements of the eyes obtained, but that the direction of these movements bore a relation to the portion of the area stimulated. This is the reverse of Ferrier's results, who got no movement from the occipital cortex, and a refinement of the results of Luciani and Tamburini, who obtained a simple conjugate deviation of the eyes. The regions from which movement of the eyes can be gotten by stimulation of the cortex in and about the occipital lobe are named in the preceding abstract. This area is divided by S., according to his results, into three zones—an upper, middle, and lower, enumerated from above downwards. The parts about the parieto-occipital fiss. form the upper; the inferior zone comprises the whole inferior surface of the lobe and the lowermost parts of the convex and mesial surfaces; while the middle zone lies between these two extremes. An excitation of the superior zone causes movement of the eyes downwards; of the middle zone, a lateral deviation, and of the inferior zone, a movement upwards. It is therefore inferred that the superior zone is connected with the upper lateral portion of the corresponding half of each retina, the middle zone with the middle portion, and the lower zone with the lower portion. S. concludes: "If we imagine the visual areas of the two cerebral hemispheres to be united in the middle line, we may conceive each retina as projected in its normal position over the united area. It will then at once appear that the upper and lower parts of both retinas will fall upon the corresponding parts of the united area, that the outer part of the left retina and the inner part of the right will fall on the outer portion of the left side of the united area, and *vice versa*, and that a vertical line bisecting each retina will fall along the line of union of the two cerebral visual areas. The parts concerned with direct or central vision will therefore correspond with a part of the mesial surface, and each pair of 'identical points' of the retinas will correspond with one and the same spot of the cerebral surface."

Ueber die Entwickelung der Furchen und Windungen des menschlichen Gehirns. J. MINGAZZINI. Moleschott's Untersuchungen, XIII, 6, S. 498. Reviewed in Centralblatt f. Physiol. No. 5, 1888, by Ziehen.

The chief point of this investigation was to determine at what stage of development the variations in the convolutions became apparent, whether they appeared simultaneously on both sides, and other related facts. 42 foetal brains hardened in alcohol and zinc chloride were the material used in the study. M. finds that almost all the variations occur between the seventh and tenth months of foetal life—quite what is to be expected when it is recollected that the main fissures and sulci alone are marked out in the seventh month. Certain sulci are not simultaneously developed on both hemispheres. For example, the fiss. occipit. II appears more often first on the left side; on the other hand the sulci orbitales and supraorbitales first on the right side. The frontal sulci appear with perfect regularity first on the right side. The growth in the length of the sulci is unequal on the two sides, and of the secondary sulci some appear between the seventh and eighth months, others between the seventh and ninth months, while the tertiary sulci appear between the seventh and tenth months.

Differences between male and female show in the development of the gyri from the eighth month on. These consist in the male not only in an absolutely greater cerebral surface, but also in a relatively greater growth of the parts lying in front of the central fissure as compared with those lying behind it.

Ueber die Lymphwege des Gehirns. M. J. ROSSBACH und E. SEHRWALD. (Centralbl. f. d. med. Wissenschaften, 1888, Nos. 25 und 26.) Abstracted in Centralbl. f. Physiol. 1888, No. 12, by Obersteiner.

It has been suggested here and there of late that the stain produced by Golgi's method for bringing out the ganglion cells depended on a deposit of silver or mercury salts in lymph-spaces. The work of these authors goes far to support such a view, and they interpret their results as showing them three sets of lymph-spaces in the brain, those about the vessels, about the nerve cells, and about the glia cells. The relations of these spaces to the perivascular spaces and to one another are such as have been described for the prolongations of the respective sorts of cells.

It should be added in support of the view here taken, that by this same reaction the authors have been able to demonstrate lymph-spaces in many other organs, as the intestine skin, liver, muscle, cartilage, etc.

Etwas über Schädel-Asymmetrie und Stirnnaht. M. O. FRÄNKEL. Nuerolog. Centralbl. No. 15, 1888.

It is certainly still open to discussion how far the development of the brain is associated with that of the skull, and whether it is safe to infer from a deformation of the skull a corresponding variation in the brain. By the younger Italian school, asymmetry of the skull is considered as a degenerative change, and their statistics go to show that it is a marked characteristic of the criminal class. Other authors look upon a moderate amount of asymmetry of the skull as

quite normal. As concerns the brain, Broca has remarked that the asymmetry of the convolutions is the special advantage of man and the more highly developed animals, while the convolutions in the primates, negro, and idiots, tend to become more and more symmetrical. Such ideas as these are of course quite out of harmony with those of the Italian school. For the purpose of seeing whether the skulls of the lower animals corresponded with their more symmetrical brain development, the author studied the relations of the frontal suture on many existing and some extinct species, and found all plainly asymmetrical, and some so to a very considerable degree. It appears from this study that brain and skull are not so interdependent, and further, that there may be some reason for considering asymmetry as the rule in the development of animal structures, and that when the Italian school point to the asymmetry of the skull as a characteristic of the criminal class, the abnormality really lies in the excessive development of the difference between the two sides rather than in a departure from perfectly symmetrical growth.

Kraniometrie und Kephalometrie. Vorlesungen gehalten an der Wiener Allgemeinen Poliklinik von M. BENEDIKT. Mit 36 Holzschnitten, viii und 172 S. Wien und Leipzig, Urban und Schwarzenberg, 1888. Reviewed in Neurolog. Centralblatt, No. 10, 1888, by Sommer.

The author has first to call attention to the relations between the atypical development of the skull and abnormal brain functioning, while the final goal of craniology is from the study of the form of the skull to infer all the laws of its growth. In his own studies he has used an elaborate instrument called an optical kathetometer. From his investigations, he is led to the view that the exterior of all skulls presents a definite number of spherical surfaces, often with very various radii; that these stand in relation to definite portions of the brain, and that between these two there is a fixed relation of growth. To determine the centres for the spheres which these surfaces represent, and to compare the changes that these centres experience with the growth of the individual, etc., are, according to Benedikt, lines of research which would be very profitable, but which he has not followed. Among the special points which he has made out are that in cases of congenital (or early acquired) blindness, there is a noticeable shortening of the interparietal arch; in congenital aphasia, stenokratophy, in deafness, a shortening of the temporal arch; in epilepsy, a deformation of the parietal bones, and in criminal and psychopathic individuals a flattening of the frontal bone. Finally, he discusses the methods for determining the capacity of macerated skulls, and finds no method which is thoroughly satisfactory.

Ueber die Erregbarkeit einzelner Faserbündel im Rückenmark neugeborener Thiere. W. BECHTEREW, in Kasan. Neurolog. Centralbl. No. 6, 1888.

In attempting to test the function of different bundles of fibres in the spinal cord, the author has hit on the happy idea of using newborn animals. As is well known, only a portion of the bundles of fibres in the cord are medullated at birth. Bechterew assures him-

self that those which are non-medullated are neither irritable nor conductive, and, further, that by using weak electrical stimuli, the escape of the current, so much talked of, in such observations, is to be little feared. These facts being accepted, the results have much value.

He experimented on puppies without any anaesthetic. In new-born puppies, the lateral and anterior portion of the posterior columns, the so-called root portion of the columnae cuneatae, the fundamental tracts in the anterior and lateral columns, and the direct cerebellar tract, are alone medullated. About five days after birth, the columns of Goll become medullated, and eleven to thirteen days after birth the pyramidal tracts acquire their sheaths.

Stimulating the cut section of the cord in new-born puppies, in the region of the root fibres of the cuneate columns, produces a contraction of the muscles which are innervated by motor nerves arising at that level, much the result one would get from stimulating the posterior nerve roots of the region.

Stimulation of the columns of Goll, five days, later produces reflex contractions of the head, trunk and limbs, as in the adult, but without any indications of pain. It will be seen that the reaction is more diffused in this second case. The stimulation of the antero-lateral fundamental tract in the caudal portion of the cord, the section having been made in the cervical region, produced contractions in the fore and hind limb of the same side, and in the tail, thus indicating the connections of these fibres.

Stimulation of the central end of the cord in the anterior part of the lateral region gave indications of centripetal fibres in this region, probably the tract described by Bechterew and Gowers, while the stimulation of the direct cerebellar tract gave characteristic movements of the head and trunk.

Not only, therefore, are these separate bundles in the cord each excitable, but each has a more or less distinct reaction.

Ueber die centralen Endigungen des N. vagus und über die Zusammensetzung des sogenannten solitären Bündels des verlängerten Marks. W. BECHTEREW. Wjestnik psychiatrii i nevropatologii, 1888, V. 2, Russisch. Reviewed in Neurolog. Centralbl. 1888, No. 10, by P. Rosenbach.

For the study of the terminations of the vagus fibres in the medulla, Bechterew found fœtuses about 28 cm. in length best suited, because at that stage of development it is mainly the nerve roots that are medullated, and these can then be easily followed by Weigert's method. The vagus fibres take several courses within the medulla. A considerable portion goes direct to the vagus nucleus. Another portion crosses the middle line to the N. ambiguus of the other side, while some fibres end in the N. ambiguus of the same side. Finally a portion goes to the Funiculus solitarius of the same side. The fibres entering the funiculus after a time emerge from it, and crossing the middle line, appear to end in a group of cells which lies mesial of the hypoglossus roots, dorsad of the inferior olive, and is longitudinally co-extensive with the hypoglossus nucleus. (This nucleus was described by Misslawski, see Neurolog. Centralbl. 1886, p. 560, and was stated by him to be the most important reflex centre for respiration.) The only other fibres which enter into the F. solita-

rius belong to the glossopharyngeus, and are stated to rise from a group of small cells which lies mesial and cephalad of the F. gracilis, at the level of the superior pyramidal decussation.

Beiträge zur Kenntnis des Centralnervensystems von Lumbricus. BENEDICT FRIEDLÄNDER. (Berlin.) Zeit. f. wiss. Zool. Bd. 47, pp. 47–83, pl. IX, X, September, 1888.

The author's chief attention was directed to the relationships and not to the ultimate structure of the histological elements. Of the three problematical "neural canals" or "giant fibres" of Leydig, the two lateral ones are shown to be directly continuous with the processes of ganglion cells at the posterior end of the ventral nerve cord, while all three at the anterior part are connected with common nerve fibres. The sheaths of these three structures are composed of connective tissue fibres, and are not comparable to the medullary sheath of vertebrate nerve fibres: the contents is a homogeneous plasma that may be squeezed out in elongated masses. Each lateral "giant fibre" receives the ascending processes of several large bipolar ganglion cells lying ventrally in successive ganglia near the posterior end of the cord, while its most posterior connection is with the similar process of an unipolar ganglion cell. These ascending processes of ganglion cells are connected with one another by transverse processes, and these in turn with the median "giant fibre." Though thus composed of fused cell processes, the "giant fibres" appear quite homogeneous in all the best preparations, and no indication of such complexity of structure as that claimed by Nausen could be obtained, though the author will not deny that such structure may exist. Artificial and deceptive results, due to imperfect means of hardening, are common and difficult to avoid. The "giant fibres" are undoubtedly nervous structures of unknown function; their sheaths may have acquired a secondary importance as aiding in stiffening the ventral nerve cord.

E. A. A.

Histologische Untersuchungen über das Nervensystem von Amphioxus. E. ROHDE. Zool. Anzeiger XI, 190; Vorläufige Mittheilung. Abstract in Çentralbl. f. Physiol. No. 10, 1888, by Paneth.

This paper gains much interest from the recent description of colossal nerve fibres in the ventral cord of Lumbricus and other worms. The central nervous system consists, in Amphioxus, of cells which lie near the central canal, and of fibres surrounding them. The supporting substance is formed by the basal prolongations of the ependyma cells. In some cases the prolongations are branched, forming a fine network. The ganglion cells are uni-, bi-, or multipolar, the last being most numerous. Their prolongations form the external nervous substance, which is mainly constituted of fine longitudinally coursing nerve fibres, in which dichotomous divisions are frequent. There are, moreover, a number of very large fibres, definite in both number and position, which arise from large multipolar ganglion cells. These prolongations are of two kinds; all except one lose their size by repeated divisions and form fine longitudinal fibres, while this one passes without any diminution in size, caudad to the other end of the cord. The paired fibres from the anterior end arise from twelve ganglion cells. There is an

analogous arrangement of cells and fibres in the posterior portion of the cord, save that the number of cells is fourteen, and that the large fibres coming from them pass cephalad to the anterior end of the cord. In this course they give off fine branches, but undergo little diminution in size, and finally terminate rather abruptly at the head end. (The failure of some of these colossal fibres to diminish in size during their course is a fact that needs further study.)

Functional Nervous Diseases, their Causes and Treatment. Memoir for the Concourse of 1881–1883, Académie royale de médecine de Belgique, with a supplement on the anomalies of refraction and accommodation of the eye and ocular muscles. GEORGE T. STEVENS. New York, D. Appleton & Co., 1887.

The title of this book is quite misleading, for the discussion of functional nervous diseases and treatment is almost exclusively limited to the relations which abnormalities of the eyes and the ocular muscles may hold to them. The author has particularly noticed that the eyes are abnormal in a large number of cases of functional nervous diseases, and further has found it possible to cure and relieve many of them by treating the eyes. He recognizes that unstable nervous systems are found, that the condition of instability may be hereditarily transmitted, and that the irritation proceeding from disordered eyes may be a stimulus strong enough to produce a functional disturbance in an unstable nervous system, without, perhaps, making it very plain by what he says that any strong stimulus may produce the same result, and that the instances which he presents are to be considered as special examples of this well recognized fact.

Studien über den feineren Bau des Geschmacksorgans. FRIEDRICH HERMANN. Erlangen, 1887. Druck der Universitätsbuchdruckerei von E. Th. Jacob. 8vo, SS. 41.

The first part of this monograph is occupied with an historical review and critical discussion of the results and conclusions reached by various investigators respecting the more intimate structure of the taste-bulbs. The remaining portion contains the results attained by Hermann, who confined himself, almost exclusively, to an examination of the foliate papilla of the rabbit. The supporting cells of the taste-bulbs, he says, are not flat cells, as supposed by some previous observers, but are spindle-shaped cells filled with fluid. They are of two kinds, inner and outer supporting cells. The outer cells, which he designates "pillar cells," and which constitute the true supporting element of the bulb, are pyramid or spindle-shaped cells, having their basal ends divided into a number of fine processes. The cell-body is marked by a distinct network of fine meshes. The nucleus is situated in the lower half of the cell-body, and contains two or three nucleoli. The inner supporting cells, which are fewer in number than the preceding, are cylindrical in form, having enlarged bases which break up into fine processes. The peripheral end of these cells does not bear needle-shaped processes. The nucleus is elliptical and lacks true nucleoli. These cells, Hermann thinks, may be those described by Schwalbe as "staff cells," and supposed by him to be sensory in function. Hermann describes a third kind of supporting cell, flat or conical in

shape, and which rests upon the mucous membrane at the base of the bulb. These cells, of which there are from two to four in each bulb, he calls "basal cells of the buds." They are furnished with an oval nucleus, and send out many delicate processes which divide dichotomously, and, by means of the network thus formed, are in connection both with each other and with the stroma of the mucous membrane. In transverse sections through the bulbs, the basal cells are seen to form a protoplasmic net, in which the author sees an analogous formation to the olfactory mucous membrane. In the stroma underlying the bulbs are dense fasciculi of very fine nerve-fibrils, which disappear in the protoplasmic net of the basal cells. Within the bulbs frequent examples of nuclear division are present. Karyokinetic figures were seen most frequently in the basal cells, and very rarely in the "pillar cells." Hermann, from this fact, ascribes to the "basal cells" the rôle of acting as compensating cells for the taste-bulbs. The granular masses of v. Vintschgau he looks upon as degenerate "pillar cells." Respecting the taste-cells, he adds but little to what is already known. The number of these cells, he thinks, has been underestimated, there being, according to his statement, from ten to fifteen nerve-cells in a bulb. Passing from the gustatory pore inwards, he recognizes a second circular opening (within which may be seen the peripheral terminations of the "pillar cells"), for which he suggests the name "inner gustatory pore." F. T.

Untersuchungen über die Papillae Foliatae et Circumvallatae des Kaninchen und Feldhasen. O. Drasch. Abhandl. d. K. S. Gesellsch. d. Wiss., Bd. XXIV, S. 231-252. Mit 8 Tafeln.

In a former memoir (*Sitzb. d. k. Akad. d. Wiss. Wien*, Bd. 88, Abth. III, 1883) Drasch published the results of an investigation of the intimate structure of the foliate papilla of the rabbit and hare. The present paper deals in general with the same subject, and is designed to supplement his earlier treatise on the taste organs in mammals. In the first paper Drasch made the statement, which he has since been able to confirm, that the sensory cells present in the bulbs could not be a criterion for the sum of the taste-fibres of the glosso-pharyngeus nerve. In other words, the number of nerve-fibres into which the glosso-pharyngeus divides, directly below the bulb region of the various taste organs, far exceeds the sum of all the sensory cells in those organs. Beneath the basal membrane of the secondary leaf of the papilla foliata is a plexus formed of medullated nerve-fibres. From this plexus, fibres, corresponding in number to the sum of the sensory cells, go directly to the bulbs. Other fibres, more numerous, pass between the bulbs to the epithelium situated above them. Many fibres, however, terminate in the membranous stroma. Below the bulb region, in the entire width of the leaf, is found a connected stratum of ganglion cells which contribute to the multiplication of the fibres. In addition to the foregoing investigation, Drasch noted the changes produced in a papilla when subjected to various kinds of stimuli. If a normal papilla be pressed upon by a glass rod or stroked with a brush, no secretion of the glands follows; but if a needle or bristle be introduced into a furrow and moved about, secretion takes place. Weak induction shocks applied to the surface of a

healthy papilla, or stimulation of the peripheral end of the divided glosso-pharyngeus, cause profuse secretion. Eight days after division of the nerve, the exterior of the papilla does not exhibit any important change visible to the naked eye, but stimulation of the peripheral trunk no longer produces secretion. If, however, the surface of the papilla be exposed to strong induction shocks, the glands continue to secrete for a while longer. By the fifth or sixth week no farther secretion takes place. In the case of a rabbit, investigated six months after division of the glosso-pharyngeus, the divided nerve had united, and the papilla experimented upon appeared to execute its functions quite normally. These experiments, Drasch says, "prove that in general, all gustable substances, when brought upon the taste papillae, or near them, induce secretion of the lingual glands, discharging into the furrows and trenches of the papillae. This secretion is due to reflex action, . . . and is brought about chiefly by means of the intra-epithelial plexus of nerves situated above the bulbs." "The glandular secretion serves for the washing away of dissolved gustable substances, and for continuous cleansing of the papillae. The time that elapses between touching the papilla with a gustable substance, and the subsequent secretion, must be such as to allow the substance in solution to penetrate as far as the bulbs. Yet the hypothesis, that over the entire papilla there are scattered fibres (having a free ending) which are capable of tasting is not inadmissible." F. T.

On the Auditory Labyrinth of Orthagoriscus Mola L. D'ARCY W. THOMPSON. Anat. Anzeiger, Jahrg. III, 1888, S. 93-96.

Professor Thompson found the auditory labyrinth of Orthagoriscus to differ in some respects from that of all Teleostean fishes. It hangs suspended by webs of delicate connective tissue within a wide space, continuous with the brain-cavity, as in Chimaera. A single vertical pillar of cartilage passes down across this space, within the arc of the horizontal canal. In the membranous labyrinth the following parts are distinguishable: latriculus with sinus superior, recessus utriculi, the three semicircular canals with their ampullae, and the sacculus and lagena. Six nerve-endings are visible, three cristae ampullarum, macula recessus utriculi, maculae sacculi and lagenae. The macula neglecta was wanting, and no trace of the ductus endolymphaticus was seen. No true otoliths are present, but instead the maculae are supplied with many small white otoconia, aggregated together. A few of these have a cubical crystalloidal form, similar to those of Acanthias, but most of them are round or oval, rough on the surface, and concentrically striated within. The proportions of the labyrinth are unusual, the semicircular canals being disproportionately long and the vestibule very small. Orthagoriscus differs from all other fishes except Lophobranchii in the complete conjunction of utriculus and sacculus, that is, in the absence of any distinction of pars superior and inferior. F. T.

On the Fate of the Muscle-plate, and the Development of the Spinal Nerves and Limb Plexuses in Birds and Mammals. A. M. PATERSON. Quart. Journ. Micr. Sci., Vol. XXVIII, 1887, pp. 109-129, pls. VII and VIII.

The author concludes from an examination of many sections that the spinal nerves are developed from epiblast throughout their entire

length. He has traced the spinal nerves, not only the nerve-roots, but also the trunks and the plexuses, as a centrifugal growth from the spinal cord. The growth of the nerves is both interstitial and terminal. They consist at first of rounded cells, in an active state of proliferation; in older embryos these become ovoid, and finally fusiform. These fusiform cells, by the alteration of their protoplasm, become converted into nerve-fibres. The development of the nerves in the limbs takes place as follows: The primitive nerve grows out beyond the lower end of the muscle-plate, and reaches the root of the limb. It there spreads out into an irregular series of processes, which pass into the undifferentiated tissue of the limb. These branches, later, arrange themselves in two trunks, one dorsal, the other ventral, which extend still farther into the limb and enclose between them a mass of blastema, from which the cartilaginous basis of the limb is formed. The dorsal and ventral trunks fuse with adjacent dorsal and ventral trunks to form two broad flat bands, from which, still later, the individual nerves as found in the adult are produced. F. T.

II.—EXPERIMENTAL.

Studien über Licht- und Farbenempfindung. A. EUGEN FICK. Pflüger's Archiv, Vol. 43, p. 441.

This valuable set of experiments on the limits of the light-sense and the color-sense, which were carried out with the assistance of Fräulein N. Fick, throw doubt on a number of results apparently obtained by Charpentier, and also serve to settle some other points which have been for some time in dispute. A former observation, the correctness of which was denied by Charpentier, is, in the first place, confirmed, namely, that the color of several separate small points is more readily detected than that of one of them. It is then pointed out that the results of Charpentier's quantitative experiments, in regard to the threshold for light and color, show an agreement which would be impossible if they had been conducted with sufficient care, and it is shown that they are in fact erroneous. Any absolute determination of a threshold for the perception of the light and color of a small faint object in an otherwise dark field is proved to be an impossible task, owing to the very great differences of sensitiveness exhibited by different individuals, and by the same individual from day to day, or even from moment to moment, and owing also to the different degree of sensitiveness of different portions of the retina, combined with the impossibility of keeping the eye steady when looking at a dark field. The facts in regard to the latter point have been variously set down,—Aubert and Erdmann, for instance, believing that any apparent difference between the fovea and the adjacent parts of the retina is due to the more rapid fatigue of the latter, and disappears after adaptation has taken place. This Fick shows to be very far from being the case. He secured good fixation by introducing two minute bright points above and below, and looking at a point half way between them. He found that the light-sense and the color-sense present opposite phenomena; the latter is more acute and the former is less acute at the fovea than at the other portions of the macula lutea. The sensitiveness for light reaches a maximum on the temporal side of the eye, somewhere

from 7° to 15° away from the centre; it is here from ten to twenty times as great as at the fovea, which is in all cases the lowest point of the curve. Different individuals furnish curves which differ very much in detail; Fräulein N. had a "fabulous" power of detecting faint lights and colors, but even for her the sensitiveness to light outside of the fovea was two or three times greater than at it. But, for the detection of color, the general shape of the curve is reversed. All colors (if the eye has undergone adaptation by the observer's remaining for fifteen minutes in a dark room) are best perceived at the fovea. Red light has the peculiarity to be seen to be light and to be red at very nearly the same instant, at the fovea. For all other colors much less illumination is necessary to see them than to name them, even at the fovea, and beyond it the difference increases rapidly.

Charpentier stated that the color of a group of points can be named sooner than they can be counted, and exactly four times sooner for all colors. Fick found that so simple a rule is far from holding; a small number of yellow points, for instance, were counted with six times less illumination than was necessary for distinguishing their color. C. L. F.

Ueber das Verhalten der normalen Adaptation. TREITEL. Graefe's Arch. f. Ophth. XXXIII, 2, p. 73.

Aubert found that the sensitiveness of the eye was increased 35-fold by remaining for two hours in a darkened room. Landolt found that with increasing adaptation, the order in which the colors were recognized was green, yellow, red, blue, violet. Treitel, by first blinding the eyes, obtained a difference of visual power of 120-fold. After fatiguing by different colors, the order of recovery for the different colors was as follows:

After Red-fatigue . . . . G, B, Y, R.
" Blue " . . . . Y, R, G, B.
" Green " . . . . R, B, Y, G.
" Yellow " . . . . R, B, G, Y.

The fatigue must take place in the retina, for it proceeds in the two eyes independently of each other. The fovea is much slower in recovering than the lateral portions of the eye. The coincidence between the time which is required for adaptation to take place and for the visual purple (or rod-purple, as it ought to be called) to become restored, points to a connection between the two processes. This is confirmed by the fact that symptomatic night-blindness occurs with diseases of the eye which attack the pigment-epithelium. Idiopathic night-blindness is always a result of excessive exposure to light; in a poor state of nutrition, among old people, for instance, a slight exposure is sufficient to bring it on.

Die Analyse der Lichtwellen durch das Auge. A. GÖLLER. Du Bois-Reymond's Archiv, 1888, I and II, p. 139.

If natural white light is first polarized, then passed through a thin piece of quartz, and then examined by an analysing prism, it will be found that the quartz has had the effect of rotating the plane of polarization, but by a different amount for the different colors. If

the piece of quartz is very thin, the whole spectrum is run through by a rotation of the analyser of less than 180°. This is rotatory polarization, and it is by rotatory polarization that Göller explains the analysis by the eye of ether vibrations into sensations of differently colored lights. Monochromatic light, on entering the eye, passes through the transparent retina, and is reflected back from the pigment-epithelium in a state of plane polarization. The outer members of the cones play the part of the piece of quartz—they shift the plane of polarization by a definite angle. A molecular motion, of much slower period, is then set up in the protoplasm of the inner members, and it is the sensitiveness of the nervous filament to the plane of this motion which constitutes the sensation of color. Two complementary colors are colors which have had their planes of polarization rotated one ninety degrees more than the other, at the same time that the phase of one has become a quarter wave-length behind that of the other, the amplitudes being the same. These conditions, Göller says, would be sufficient to cause their superimposed harmonic motions to produce the motion of circular polarization, and that would be indistinguishable from the motion produced by all the colors of white light acting together. Two vibrations whose planes were at a different angle, would give an elliptic motion of such a kind that the direction of its major axis would give its tint, the excess of the major axis over the minor would give its saturation, and the minor axis would give the amount of white light mixed with it. No two colors other than those described above could give white light, for motion in a circle can only be produced, Göller says, by two rectilinear motions of the same amplitude and at right angles to each other, and thus is explained the fact that most colors when mixed in no matter what proportion cannot be made to produce white light. This exposition seems to contain two grave errors, both depending upon the fact that Göller has apparently overlooked the effect of the period upon the composition of harmonic motions. Two harmonic motions at right angles to each other cannot produce circular motion unless their period is the same; and to suppose that the outer members of the cones, besides the difficult functions already assigned to them, had also the power of equalizing the periods of all rays transmitted by them, would be much too forcible an assumption. Again, it is not true that two simple harmonic motions which are not at right angles to each other cannot produce motion in a circle, if the right difference of phase is chosen to fit their inclination, any more than it is true that the square is the only parallelogram in which a circle can be enclosed. This theory, therefore, which looks rather interesting at first sight, would seem to be utterly untenable. It is possible that some polarization theory might be proposed, with a somewhat different set of assumptions, which would stand examination; the chemical theory is far from being so well established as it is commonly assumed to be. The assumption that there are colorless visual substances, which act after the rod-purple is bleached, and in those places where it never existed, is a purely gratuitous one, and not the slightest reason has been brought forward to support it. C. L. F.

Ueber optische Bewegungsempfindungen. Prof. SIGMUND EXNER. Biologisches Centrablatt, 15 September, 1888.

In this lecture, Prof. Exner brings together so many interesting observations upon a topic to which he has largely contributed, that a somewhat detailed abstract will be desirable.

He begins by introducing into the sensation of motions a distinction between the inference of the motion of a body from the fact of seeing the body in different regions of space at successive intervals, and the immediate perception of motion as a simple elementary sensation. The distinction comes to the front in the observation, originally due to Czermak, that the second-hand of a watch, if observed in indirect vision, seems to move very much more slowly than when directly viewed. But Prof. Exner thinks that in the former case the motion is not perceived, but inferred; in the latter it is directly perceived. To show that it is not necessary for a perception of motion to have the object seen in two successive positions, one need only have the space between the two points, or the interval between the two appearances, so minute that they cannot be distinguished, and yet have a sensation of motion as the result. Under certain conditions two impressions succeeding another with an interval of .045 second are just recognized as distinct in time; but the *direction* of the motion of a light under the same conditions can be perceived when the interval between the beginning and end of the motion is only .014 second. But one can reduce the distance so that the beginning and end of the motion are no longer distinguishable. This is especially easy on the lateral portions of the retina, where one finds that two disks so near together as not to be seen as two, are none the less seen to move with the slightest motion, the lateral portions of the retina being very sensitive to motor sensations. If a row of dots be viewed away off in indirect vision, the number or distances of the dots will not be seen, yet the addition or removal of a dot will be noticed instantly.

This sensation of motion as distinguished from the inference of motion has a lower limit. Aubert found that a motion slower than 1°–2° per second is not felt, which amounts to about a distance of 6–7 cones on the retina. Like ordinary sensations, these motor impressions leave an after-image. If a disk upon which a spiral is drawn be rotated, one will get the impression of a point moving towards the centre; if the disk be suddenly stopped, one sees a motion in the opposite direction. So, too, if after viewing such a rotating disk one casts the eyes on any object, that object, or that portion of it falling on the portion of the retina formerly stimulated, will be distorted, showing that these effects are retinal and not motor. If one views a disk with marked sectors rotating slowly, through a rotating disk with sectors cut out, one can so regulate the speed of rotation that the black sectors will not seem to be moving at all, being thrown back by the after-image upon the interruption of the sensation in the other disk.

That the after-image is confined to the portion of the retina formerly stimulated is shown by Dvorak, who had the different portions of his spiral rotating in different directions, and obtained an after-image corresponding to this difference. So, too, Fleischl found that after obtaining an after-image of a point moving horizontally, the projection of it on a series of vertical lines extended only as far as the original motion.

What will be the result if the two eyes have different motor-sensations? If you regard a rotating disk directly with one eye, and through a reversion prism with the other, you see opposite directions of rotation with the two eyes, giving rise to an uneasy feeling and no distinct after-image. If you close one eye you get its appropriate after-image. But most curious of all, if you look at the disk with one eye until fatigued, then close and look at a white surface with the *other* eye, you will see an after-image of the disk rotating in an opposite direction.

This holds equally well for the third dimension. A wheel rotating in the median plane is seen in the third dimension, and when suddenly stopped, the after-image is also seen in perspective. But this is obtained by a combination of the different after-images of the two eyes. A true after-image in the third dimension is not obtained. In the after-image only that portion of the nervous system is involved that aids in the perception of the adjacency of space impressions. In riding in the rear car of a train and looking backwards we see objects hurrying away from us. If the train stops we seem to be approaching the objects. In the former case the retinal impressions gradually grew smaller; now they by the after effect grow larger, and thus lead to the inference of our approaching them.

Again, it is found that if there is no stationary object in the field of vision, the minimum perceptible rate of motion is much raised; the threshold for motion becomes 10 times as high. It makes a difference whether the object moves across the retina or the eye follows the object across the field of vision; in the latter case the motion seems only about half as rapid as in the other. We have a more accurate notion of the motion of images on the retina resulting from the viewing of a stationary object while the eyes move, than we have of the motion of the eye muscles. If there is no object in the field of vision recognized as stationary, the perception of motion becomes vague; so in a dark room the movement of a light could hardly be seen.

We distinguish then between a sensation of motion which is immediate and is probably a subcortical function, and the conscious perception of motion by inference from various sensations.

Experimentalle Untersuchungen zur Amblyopiefrage. Dr. F. C. Müller-Lyer. Arch. f. Anat. u. Phys. 1887, p. 400.

Setting out with the idea of studying the phenomena of sight disturbed by disease, by studying normal sight as disturbed by experimental conditions, the author of this lucid article investigated four points, namely, (1) discriminative sensibility, (2) sharpness of vision, (3) color-sense, and (4) extent of the field of vision toward the periphery, in the three following conditions: (*a*) Simple weakness, (*b*) state of stimulation, (*c*) state following stimulation. Simple weakness of the eye can be paralleled for experiment by weakening the stimulus. The study of the first point under this condition is simply a retesting of Weber's law. After a set of careful experiments, the author found, as others have done, that Weber's law is not strictly exact. The discriminative sensibility is not constant, but depends on the intensity of the stimulus. The nearest mathematical expression for it (and that only an approximation) is that the former varies as the cube root of the latter. The second point was

tested with printed letters, with the result of finding that the sharpness of vision follows in its decline essentially the same curve as the discriminative sensibility, but declines a little more slowly at first and a little more rapidly at last. The experiments on the third and fourth points coincide with the usual results, namely, that the limits of vision for green and red are first contracted, then those for yellow and blue; and the colors are finally lost in the same order, while the field for white remains uncontracted even with very considerable darkening. When the eye is stimulated from a source of light between itself and the object upon which it is fixed (condition *b*), it is found that when the extra stimulation increases in intensity, the discriminative sensibility declines more rapidly than the sharpness of vision, and that the disturbance of vision increases as the illumination of the fixated object is reduced, showing the eye thus stimulated to be delicately hemeralopic. Under such extra stimulation the visual field for white is concentrically contracted, the contraction depending in its amount, while the extra stimulus is constant, on the illumination of the object. The colors have their fields contracted and disappear in the order of their brightness, though this is not that of the extent of their fields in normal vision; blue, which then has next to white the widest field of all, may, under the influence of the extra stimulation, disappear, while all the other colors, except violet, are still to be seen. The results for the eye after stimulation (condition *c*) agree with those just given for condition *b*, except that with daylight illumination of the object fixated, colored vision is introduced (especially red-seeing and green-seeing), which brightens one color to the disadvantage of its contrasting color. For the diseases of vision with which the above conditions are comparable, as for the details of the apparatus and methods used in the experiments, the article itself should be consulted.

Die Umkehrung des Sehens und des Gesehenen mit Beziehung auf die gleichzeitige Seh-Abprägung. Prof. HOPPE. Pflüger's Archiv, XLIII, 1888, p. 295.

The "conversion of relief" in plane drawings, as in that which appears to be a half-open book, now seen from behind and now from in front, or like the Schroeder stair figure, has generally been explained as due to a change of conception in the mind of the observer, or to that helped out by ocular motion. Prof. Hoppe finds an additional factor in differences of the impression (*Abprägung*) of the image on the *macula lutea.* He presses the nativistic argument so far as to suggest that the *macula lutea*, in a certain way and to a certain degree, *knows* its own images. For proofs of his position the article itself must be consulted.

On Wundt's Theory of Psychic Synthesis in Vision. J. H. HYSLOP, Ph. D. Mind, XIII, p. 499, Oct. 1888.

After a preliminary explanation of the apparent location of stereoscopic images, Dr. Hyslop quotes Wundt's theory of psychic synthesis with qualified approval, and gives several interesting experiments (for the most part given in his letters to *Science* in the early part of this year) that in a measure confirm that theory. He finds in it, however, a confusion of two conceptions of innervation; the first

making innervation very closely associated with actual muscular contraction and the discharge of nervous energy, the second being little more than the volitional impulse. The author's chief criticism is directed against the first conception. He points out complications into which the theory is driven when it tries to show why innervation of the internal and external recti for different degrees of convergence should give the notion of differences of distance in the third dimension, while that of the other ocular muscles, or even of the recti themselves for parallel motion, gives nothing of the kind. The difficulty of accounting by this theory for the localization at the same time of a pair of homonymous and a pair of heteronymous images is also urged. Since their place depends on innervation, there would have to be innervation at the same time and of the same muscles for different distances. Moreover, it can be shown by experiment that localization may vary with attention, while the position of the eyes and, presumably, the innervation that controls them, remains the same. Against the other form of the theory is urged that it makes a useless distinction of central and peripheral sensations in distinguishing those of innervation from others when all are really central.

Geschmacksprüfungen. KARL RITTMEYER. pp. 28. Göttingen Diss. 1885.

After reviewing the various opinions regarding the portions of the mouth cavity capable of perceiving taste, Rittmeyer made an independent investigation, thoroughly cleansing the tongue after each test, and avoiding contact with the edges of the tongue. He experimented upon ten persons, and found in every case a sensibility to taste outside the tongue—properly and especially (if not exclusively) upon two regions, a portion of the soft palate and the arcus glossopalatinus. Denoting a very pronounced taste sensibility by 1, a minimum sensibility by 4, and with 2 and 3 intermediate, the results for the four cardinal tastes in the average of ten persons were as follows:

For *sweet*, in nine cases the root of the tongue was 1, the edge 2, the tip 3, the soft palate 3-4, the arcus glossopalatinus 4. In one case the tip was 1, the edge 2, and the root 3.

For *salt*, in five cases the result was precisely the same as for the nine cases with sweet; of the remaining five cases, two differed merely in marking the soft palate 4 instead of 3-4, two differed by conforming to the exceptional instance with sweet, and one differed by marking the tip 4 and the soft palate 3.

For *sour*, the root of the tongue is marked 1 twice, 2 three times, 3 four times, and 4 once. The edge is marked 1 seven times and 2 three times. The tip is marked 1 once, 2 four times, 3 four times, and 4 once. The soft palate, 3 twice, 3-4 once, 4 seven times. The arcus glossopalatinus, 3-4 once, 4 nine times.

For *bitter*, the root is marked 1 nine times and 2 once. The edge, 2 nine times and 3 once. The tip 1 once and 4 nine times; the soft palate, 2-3 ten times. The arcus glossopalatinus, the same. This would make the root of the tongue best for sweet and bitter, the edge best for sour. Besides minor variations, one of the ten individuals shows a decidedly different distribution of sensibility from the other nine.

Next, the connection of the organs of taste with the nerves is discussed and illustrated by pathological instances. All agree in making the glossopharyngeal the taste-nerve for the root of the tongue, but the opinions vary regarding the connections of the anterior two-thirds of the tongue.

Next, the effect of various drugs upon the taste organs was tried, with the result of showing that *alumen crudum* and *zincum sulphuricum* in solutions of 1.25–1.5 per cent had the most decided effect. Testing the same subjects as before, and denoting a slight weakening of the sensory effect by A, a stronger one by B, a very strong one by C, and a total absence of taste by O, we have the following result:

For *sweet*, the root is marked A twice, B eight times. The edge is marked B once, C three times, and O six times. The tip, C once, and O nine times. The soft palate and arcus glossopalatinus each, B once, C once, and O eight times.

For *salt*, the root is marked A three times, C seven times. The edge, A five times, B four times, and C once. The tip, C ten times. The soft palate, B five times and C five times. The arcus glossopalatinus, C ten times.

For *sour*, the root was marked A once, B once, and C eight times. The edge, A five times, B twice, and C three times. The tip, A once, B seven times, and C twice. The soft palate and arcus glossopalatinus each, B seven times, and C three times.

For *bitter*, the root was marked A ten times. The edge, A twice, B seven times, and C once. The tip, O ten times. The soft palate, A once, B twice, and C seven times. The arcus glossopalatinus, A twice, B eight times. In other words, the root of the tongue loses its perception of taste least under the action of drugs, the edges next, then the soft palate and arcus glossopalatinus, and most readily the tip. Again, the root retains best its taste for bitter, next for sweet, third for salt, and last for sour. The edge retains the other three tastes about equally well, but is most liable to lose all taste for sweet. The tip retains best what it tastes best, the sour, but loses absolutely what little taste for sweet and bitter it normally has, retaining a slight taste for salt.

Finally, the application of a 2 per cent solution of cocain, besides inducing anæsthesia, does away with all taste for as much as half an hour, and longer if the application is allowed to remain. The taste for salt and bitter seems to come back first, that for sweet last.

Action des acides sur le goût. JOSEPH CORIN. Archives de Biologie, VIII, fasc. 1.

The relation between chemical action and sensation of taste is at the simplest with acids. Substances that taste acid are chemically acid. The investigation of this relation is the object of this very commendable research. When the sense of smell is excluded by holding the nose or by using very weak solutions of acid, the following have almost exactly the same quality, though different intensities of taste, and were used for experiment, namely, chlorhydric, phosphoric, oxalic, formic, sulphuric, acetic, nitric, tartaric, citric, hypophosphorous, malic, and lactic. Precautions were observed in making comparisons, to operate always on the same part of the tongue (the tip), to use the same quantity of acid, and to allow the

same time for judgment. The tongue was used only when free of the effects of eating, drinking, or smoking, and was carefully rinsed. It was found best to test but a few substances per day, and these were of course varied in order, and arranged to avoid prejudgment. The number of taste organs stimulated, which Camerer found to affect the number of right guesses, does not seem to have been fixed beyond the using each time of the same quantity of the solution. In trying to fix the weakest solution of acids that could be distinguished from pure water, the experimenter found that with the same acid it varied at different times, probably with his own condition, from 3 to 35 parts in 10,000. It is possible, however, to compare weak solutions of the same or of different acids with a good deal of exactness if the experiments are made as nearly as may be at the same time. It was found that for solutions of chlorhydric acid ranging in strength from 15 to 25 parts in 10,000, a difference of 6 parts was distinctly perceptible; under exceptional circumstances, for solutions containing between 3 and 15 parts of acid in 10,000, a difference of 3 parts could be recognized. The portion of the research that bears directly on the connection of tastes with the chemical character of the acids, consisted in arranging variously proportioned standard solutions of the different acids in the order of their sourness. The results were consistent for both mono and polybasic acids, and are as follows: (1) the intensity of the acid taste is not the same for all the acids at the same degree of dilution, *i. e.* the same weight of acid diluted with the same weight of water; (2) the intensity of the taste is not proportional to the amount of replaceable hydrogen in the solution; (3) the taste of solutions containing each the same number of molecules of acid is stronger as the weight of the molecule is less. Whence it is concluded, (4) that "the intensity of the acid taste of a molecule of any acid depends on the relation of the weight of acid hydrogen contained in the molecule to the weight of the molecule." The order of the acids thus arranged is that given above. The experiments were all made by the author upon himself, and he recognized an educative process from the experiments in his power of discrimination. E. C. S.

Beobachtungen über die Geschmacksempfindungen nach der Zungenexsterpation. N. Cybulski and A. Beck. Transactions of the Academy of Sciences of Cracow, 1888; noted in Centralbl. f. Physiol. No. 12, Sept. 15, 1888.

These experimenters found in a patient whose whole tongue, including the basal taste papillae, had been removed, that there yet remained some ability to taste. The sensations of sweet, bitter and sour could be caused by touching the back of the throat or the mucous surface of the stump of the tongue with appropriate substances, though in the latter case they were only perceived when movements of swallowing were made. The taste of salt could not be excited.

Die Einwirkung der Kohlensäure auf die sensiblen Nerven der Haut. Goldscheider. Verhandl. der Physiol. Gesells. zu Berlin, Nov. 25, 1887.

When the hand is plunged into a vessel of carbonic acid, a sensation of warmth is felt. This increases for a time and then declines.

At the suggestion of du Bois-Reymond the author undertook an investigation of the phenomenon. After excluding more or less completely by experiment or well known physical principles the possibility of the sensation being due to the dampness of the gas, its conductivity, its heat capacity, its absorbent power, its setting free heat in its absorption in the moisture of the tissues of the skin and its causing an elevation of the skin temperature by dilation of the small blood-vessels, he concludes that it is really due to an actual chemical stimulation by the gas of the nerves of warm sensation.

Thermische Experimente an der Küchenschabe (*Periplaneta orientalis*). V. GRABER. Arch. f. d. ges. Physiol. XLI, abstract by Hermann in Jahresb. Anat. u. Physiol. Bd. XVI, Abth. 2, 1888.

The limits of temperature fatal to these roaches are —6° C. and 41°. With decreasing temperature, at about 5°, they lose locomotion and, if they remain at that temperature, other power of motion also. They will still respond, however, to strong stimulation. Below 0° they soon become paralysed, but recover more or less perfectly when warmed again. At —5° or —6° they die in from 10 to 20 minutes. Increasing temperature makes them more lively; above 37° they go into convulsions, and die slowly at 41°, though for five minutes or less they can bear 60°. Graber tested the temperature preferences of these animals by an apparatus of three connecting chambers, the two outer ones of which were of variable temperature. If the side chambers were both high, say 38°, the insects all stayed in the middle one. If they differed by about 2° and were still high, most of the animals chose the cooler. If the side chambers were both cold, they picked the warmer. The roughness and conductivity of the floor were of great influence. The "optimum" or temperature of greatest preference was about 26° or 28°, but at this very point the animals were frequently uninfluenced in their choice by wide differences of temperature. When offered a very hot chamber and a very cold one, they preferred the hot one up to about 39°, or only went into the other for a little while to cool off. When the hot chamber was yet hotter, they preferred the cold, even if below zero. Strange to say, they did not in these experiments remain in the middle chamber.

Die räumliche und zeitliche Aufeinanderfolge reflectorisch contrahirter Muskeln. Dr. WARREN P. LOMBARD. Separat-Abzug aus Archiv f. Anat. u. Phys. 1885.

To know a reflex act one must know the muscular contractions that enter into it and their order and extent in space and time. Such an analysis Dr. Lombard made for the reflex contraction of the muscles of a frog's leg. He found that the reflex called out by a continuous heat-stimulation was not a continuous contraction, but one broken by periods of rest; also that the order of contraction of the muscles in a series of reflexes was not constant; that, other things being equal, the number of muscles excited, and the length of time required for the stimulus to spread to all the motor roots, varied with the kind and intensity of the stimulus. From these he concludes that there must be somewhere in the central portion of the centripetal-centrifugal arc an apparatus that holds back the

incoming excitation till it has reached a certain intensity and then transmits it to the motor roots; and that there is independent connection between the centripetal nerve and each motor root controlling muscles that enter the reflex. The difficulties involved in the older assumption that the order of contraction is fixed by the various rates of central conduction are avoided by supposing that the order depends on a difference of excitability of the structures connecting the sensory and motor roots. The grade of excitability would depend on chemical conditions, which can change quickly and in limited areas, and so produced the variable order of contraction found. The importance of chemical conditions is apparent in the strychnized frog where the differences of the periods of delay for the different muscles are abolished.

Relation de diverses experiences sur la transmission mentale, la lucidité, et autres phenomènes non explicables par les données scientifiques actuelles. CHARLES RICHET. Proceedings of the Society for Psychical Research, Part XII.

This lengthy article of 150 pages with so startling a title, coming from so prominent a scientist, is sure to attract one's attention. In a topic where so much bad method has prevailed, one expects much from a trained scientific thinker. Unfortunately this expectation is doomed to disappointment. M. Richet's application of the theory of probabilities to his results is very shallow, and the nature of his evidence often entirely too subjective. To begin with, his subjects are four hysterical women, for whose honesty we must be satisfied with Prof. Richet's declaration in their behalf. The first test consists in his willing one of his patients to go to sleep when the latter is at a house several hundred yards distant. Upon going to the house he hypnotizes the subject, who then informs him of the time during which he attempted to will her to sleep. The experiment is varied, but the time given by the subject is in Prof. Richet's opinion so often near the truth that chance fails to account for the successes. Again, hundreds of trials are made to transfer a simple drawing from Prof. Richet's mind to that of the subject. A large number of illustrations record the more successful cases, but the new fact that is emphasized is the discovery that the reproduction was almost equally successful when M. Richet himself was unaware of the character of the drawing to be transferred. This leads him to postulate a state of "lucidity" in which mental impressions are possible without the ordinary aid of the senses. Again, he experimented with a group of sixty drawings with normal subjects, and found on the average seven successful "transfers" in two hundred trials, while with his selected subjects he obtained twenty successes in the same number of trials. The subject while in the hypnotic state attempts to describe the disease of a patient, a lock of whose hair she sees; the descriptions are vague and do not impress the unprejudiced reader as at all noteworthy. Experiments in guessing cards were tried, but the number of successes was not above what chance would account for. This only sketches a small portion of this comprehensive study, which must be read in the original with account of precautions and the illustrations of results.

M. Richet enters upon his research with what appears, in the light of a sound logic, an utterly false notion, namely, that chance or a

new force is the only explanation; entirely neglecting the great probability of our having overlooked a natural mode of explanation, such as the effect of unconscious suggestion. Again, he values the mere accumulation of evidence, as opposed to the stringency of the evidence, far too highly; and more important than all, when he comes to rule out the element of chance successes he fails of his purpose entirely. To begin with, the only type of experiment in which the success attributable to chance is exactly assignable is that with the cards, which proves entirely negative. In all other cases the action of chance is only roughly estimated, with a large element of subjectivity; and to judge from this article, M. Richet seems very readily disposed to see a marvel in every unusual event. In that portion of the article dealing with coincidences, the frequent though not the less unpardonable mistake is committed of confusing the chances of an event happening at a time determined upon *beforehand* by a third party, and the calculation of the chances *after* the event, without taking into account the prediction of the occurrence. Finally, the fact that success was obtained when the agent did not know the nature of the drawings is not an argument for "lucidity," but an argument against telepathy, and suggests that the subject succeeded in getting a sufficient idea of the nature of the drawing to obtain three times the normal number of successes.

Hat das magnetische Feld directe physiologische Wirkungen? L. Hermann. Pflüger's Arch. XLIII, 5 and 6, April 24, 1888, pp. 217–235.

The psychologic interest in this paper centres about the alleged powers of the magnet in hypnotic phenonema. Prof. Hermann attacks the problem from a purely physiological side, aiming to discover whether the presence of a strong magnetic field in any ways influences the behavior of sensitive tissue under ordinary stimuli. After calling attention to the fact that in the literature of the subject one finds only negative results, when the results are trustworthy, he recounts his own experiments, which were directed mainly to four points. (1) Is there any difference in the minimal intensity of an induction shock that will cause the contraction of a nerve-muscle preparation, when that preparation is in a magnetic field and when it is not? (2) Is there any difference in the curve of contraction of such a preparation when placed in a magnetic field and when not? (3) Is there any difference in the minimal rate of stimuli that will produce tetanus under the two conditions? (4) Will the curve of tetanic contraction differ in the two cases? To all these questions, the answer obtained from numerous experiments, made with great precaution, is entirely negative. The magnetic field has absolutely no physiological effect whatever. Basing his position on these and similar results (for animals behave perfectly normally in a magnetic field; microscopic functions continue as usual; placing one's head between the poles of a magnet results in no sensation), he launches a severe criticism against the unscientific proceedings of the "hypnotists" who attribute a marvellous influence to the magnet, under conditions anything but conclusive. He emphasizes the extreme improbability of any such result, and regards all such anti-physiological announcements as utterly untrustworthy and an evidence of nothing but the careless observation of the reporter.

Psychometrische Untersuchungen. JAMES MCKEEN CATTELL. Inaug. Dis. Wundt's Philos. Studien, III (1866), pp. 452–492 ; Mind, XI (1886), pp. 220–242, 377–392, and 524–538.

(2.) *Wundt's Philos. Studien*, IV (1887), pp. 241–250. *Mind*, XII (1887), pp. 68–74.

In his discussion of the factors of reaction times, Dr. Cattell more than foreshadows the distinction of sensory and motor reactions, which Wundt has made so important in the last edition of his Psychology. He conceives that in the reactions of practiced subjects the brain processes are not chiefly those that attend perceiving and willing, but rather a kind of voluntarily prepared reflex. "That is," he says, in speaking of light reactions, "the subject by a voluntary effort . . . puts the lines of communication between the centre for simple light sensations (in the optic thalami, probably), and the centre for the co-ordination of motions (in the corpora striata, perhaps connected with the cerebellum), as well as the latter centre, in a state of unstable equilibrium." In case, then, of an incoming nervous excitation, a part goes on to the cortex and arouses consciousness, but a part also shoots off on the prepared lines and causes the immediate execution of the motion of reaction. Dr. Cattell generally used his gravity chronometer, (*vide* §2, p. 709, Vol. I of this JOURNAL), to control the giving of the stimulus. For the signaling of the reaction he used a telegraph key, a lip key, and a sound key. The signal was made with the first by raising the finger, and in the others by calling out. The time was measured by a Hipp chronoscope. The author and Dr. G. O. Berger, both somewhat experienced in psychological experiment, acted as subjects, and care was taken not to introduce irregularity by fatigue, etc. The simple reaction time for daylight reflected from white paper was found to be for B. 0.151 s., for C. 0.147 s., reactions with either hand being about equally quick, with the vocal organs about 0.030 s. slower.

The central stages of reaction time, *i. e.* perception time (*Unterscheidungszeit*) and will time (*Wahlzeit*), cannot be measured directly ; the only safe way to study them is in their variations. If the manner of reacting remains the same, the will time should be nearly constant, and the independent variations of perception time open to study. In the first set of experiments on perception time, the subject was shown two cards, one black and the other white on a black background, and was required to react with the hand to the white alone. This gave, B. 0.207 s., C. 0.242 s. Subtracting from these the simple reaction time, on the assumption (which, however, the author makes with some hesitation) that all the processes of conduction, etc., are the same, there is left for the stages of perception and will, for B. 0.061 s., for C. 0.095 s., and dividing these equally between them (which cannot lead to gross error with such small numbers), gives for the simple perception time alone B. 0.030 s., C. 0.050 s. Calculation from the vocal reactions gives about the same results. Variations of the experiments by the substitution of colors or letters or words as stimuli, and by changes in the discriminations to be made, increased the perception time by different amounts. The perception time for pictures about one cm. square and, as the author conjectures, for the objects to which they correspond, was for B. 0.092 s., for C. 0.117 s.

The will time is studied by changing the manner of reacting. Instead of reacting to a designated stimulus in a single fixed way,

stimuli of several kinds are used and a different reaction set apart for each. If two stimuli are used, reaction may be made to one with the right hand, to the other with the left. Using red and blue and yellow and green as such pairs of stimuli, B. took 0.018 and C. 0.034 longer than when a single previously determined motion sufficed. Such experiments were also made on letters with similar results, but the most numerous were made with vocal reactions—the color was named, the word called out, etc. In this case the variations in the manner of reacting are more numerous and the association of stimulus and reaction closer. Letters, figures, colors, words, and pictures were the stimuli, and interesting variations in their times were discovered.

Dr. Cattell sums up his measurements in round numbers, in thousandths of a second, as follows: Simple reaction time for light, B. 150, C. 150; recognition time for light, B. 30, C. 50; for a color, B. 90, C. 100; for a picture, B. 100, C. 110; for a letter, B. 120, C. 120; for a short word, B. 120, C. 130; naming time for colors, B. 280; C. 400; for pictures, B. 250, C. 280; for letters, B. 140, C. 170; for words, B. 100, C. 110. The author investigated with great care, as well, the effect of attention (concentrated, normal, and distracted), fatigue (making a very long special test), and practice, concluding that the first two are of less influence, at least with practiced observers, than has commonly been supposed.

(2) The last division of this research carries still further the application of time measurements to mental action. Four of the five groups of measurements deal with association or recollection, the fifth with the time of acts involving a judgment. The first group gave the time for naming pictured objects in a foreign language (for B. 0.172 s., for C. 0.149 s. longer than to do the same in the mother tongue), and for translating German words into English and *vice versa* (from less than one-fifth to nearly three-fifths of a second longer than for simply seeing and naming a word). For the second group the subject was required to give the country when a well-known city was given, the sum or product of given numbers, the language when an author was given, etc. These associations required from two-fifths to four-fifths of a second. The third group allowed more liberty of answer. When a country was given, the subject had to reply by a city in it, or when an author was given, by one of his works, etc. The times ranged from about two-fifths to one and one-tenth seconds. The associations of the fourth group were such as a thing with its parts or its uses, class name with examples of the class, a verb with subject or object, etc. The time required was from a little under three-tenths to a little over four-fifths of a second. The element of judgment was introduced in the fifth class by requiring an estimate of the length of single lines, or of the number in groups of them, or the relative greatness of great men. The times were from about one-sixth to about one and one-eighth seconds. For the exact figures, as for interesting peculiarities of association suggested by them, the article itself must be consulted. The mental processes measured, however, are not the same even in groups of the same general form; the average variations of the times found are very large, amounting in the corrected averages not infrequently to more than one-fifth of the whole; and the number of experiments is very small, often not more than twenty-six of a kind. These points, which the author indeed recognizes, leave this part of his research with hardly any

value more weighty than suggestiveness. It may very well be questioned whether the measurements have not been pushed to more complicated processes than can yet be approached with advantage.

E. C. S.

Ueber den Einfluss der Uebung auf geistige Vorgänge. Dr. G. O. Berger. Wundt's Philos. Stud. V. 1. 1888.

The influence of practice was measured by its effect on the rapidity with which gymnasium pupils of different classes, and those of the highest class of a preparatory school, could pronounce Latin and German. The best five and the worst five in each of the classes were taken for the trial; the average age in the class from the preparatory school was 9; in the highest gymnasium class, 21.6. The test consisted in reading with the greatest rapidity first 100, then 500 words, and third, the first 100 words again at the normal rate. The Latin read was from Tacitus's Agricola; the German, from Goethe's Egmont. The improvement in the rate through the ten classes follows what may be assumed as the general law of the effect of practice, namely, a rather rapid quickening at first, followed by less and less gain as practice continues. The time for 100 words in the preparatory class, which had not as yet studied Latin, was 262 seconds; for the gymnasium classes respectively, 135, 100, 84, 79, 57, 54, 49, 48, 43. For German the times were 72, 55, 43, 37, 39, 28, 27, 26, 25, 23. The 100-word rate in Latin is 7 per cent shorter than that which can be kept up for 500 words; in German, but 3 per cent. The "normal reading" in the lower classes was a little quicker than the first reading because the words were a little familiar. The higher classes took longer for the second reading than for the first because they read for the sense. To set aside the possible objection that the increased speed was an evidence of increased mental quickness, and not the result of familiarity with the language, the gymnasium pupils were shown sets of five and of ten colors, and the time required to recognize and name them measured. The rates do not increase regularly with the increase in age, as they should do if the objection were valid. Granting the increased rapidity by practice, the question follows as to how practice has made the change. The gain appears to be chiefly in the overlapping of processes, as in Cattell's experiments (noted in the Journal, I, p. 709), and in the size of the groups of words grasped at a time. The children in the preparatory school, for example, read Latin by syllables; those a little more advanced, by words; the highest, by phrases, as is testified by the kinds of errors made in reading at full speed, and by the less proportionate advantage shown by the boys of the higher classes in reading disconnected words.

Ueber die Reactionszeit für Erregung und für Hemmung. Gad, nach Versuchen des Herrn Dr. Orschansky. Verhandlungen der Physiol. Gesells. zu Berlin, No. 13–14, June 4, 1887.

The muscle selected for these experiments was the masseter, because its relaxation is not attended by the contraction of an antagonist. Its contractions and relaxations, by means of which the reaction times for excitation and inhibition were measured, were recorded by a double-branched apparatus, one branch of which entered the mouth on each side and pressed against the muscle,

while a forward pair were adapted to recording with a Marey's drum. For comparison, the motions of the lower jaw under the action of antagonistic muscles were also experimented upon. The rhythm of most rapid contraction and relaxation proved at first slower in the masseter than in the others, but practice equalized the rate, though it seems to be effective only in shortening the stage of contraction. Such experiments, however, are not suited to determine the real reaction time for inhibition. When that was measured directly by reacting with relaxation to an electric shock, the inhibition time was found to be the same as that for excitation, and the equality continued through variations of the intensity of the shock, fatigue, alcohol dosing, etc. The simple reaction time for the jaw motions was, closing 0.15 s., opening 0.17 s.; for the masseter, contraction, before practice 0.25 s., after practice 0.15 s.; relaxation, before practice 0.30 s., after practice 0.14 s. Interesting experiments were also made on the variations introduced by the strength of the spring that pressed the arms of the apparatus against the muscle. If the subject intended to cause a slight motion and the spring was stiff, the reaction time was decreased; if under the same circumstances he intended to make a considerable motion, the time was lengthened.

Ueber Wiedererkennen. Versuch einer experimentellen Bestätigung der Theorie der Vorstellungsassociationen. ALFRED LEHMANN, Ph. D. Philos. Studien, Bd. V (1888), H. I, pp. 96-156.

Can all the phenomena of association be explained by the law of *contiguity?* This is the problem that Dr. Lehmann attempts to solve. From the standpoint of association by contiguity, recognition of simple impressions is possible, in the author's opinion, only under two conditions: first, that the memory-picture of a former sensation still exists, with which the later sensation may be compared; or, second, that a name or the like has been associated with the sense impression. The latter is not strictly recognition, but is so called. Dr. Lehmann's experiments, performed with sensations of light, cover both cases.

In the investigation of the first case, the different shades of gray, produced by means of rotating disks partly black and partly white, were employed. The disks were shown by means of a carefully prepared apparatus, in the following manner. First, a disk of normal shade was shown. After the lapse of an interval the normal disk was again shown; or a disk of lighter shade, or one of darker shade appeared. The observer judged whether the disk last shown was like or unlike the former one. In the first set of experiments only two disks were used. The interval was 30 sec. The normal disk was half black and half white, *i. e.* it $= 180°$ black $+ 180°$ white. The other disk varied between 240° white + 120° black and 188° white + 172° black. Under these conditions, as the amount of white in the disks decreased, the average number of correct answers in each series of 30 experiments fell from 29 to 18 with one observer, from 27 to 17 with the other; the number of correct answers likely to occur by chance being of course 15. Thus as the difference between the normal disk and the light disk decreases, the number of correct answers diminishes. In another set of similar experiments three disks were used; and the light disk was always as many

degrees lighter than the normal disk as the dark one was darker. Six series of 30 experiments each showed a decrease in the number of correct answers, especially as the disks approached one another in shade. Thus an increased number of possible impressions decreases the number of correct answers. Varying the time-interval, five series of 30 experiments each showed that, as the time increased from 5 sec. to 120 sec., the number of correct judgments decreased with one observer from 30 to 17, with the other from 21 to 17. Individual differences due to inclination, talent, and other personal conditions are here apparent. The effect of practice was also noticeable, the second half of a series generally showing more correct answers than the first half. Thus far, in the author's opinion, the theory of association by contiguity satisfies all demands as an explanation of the recognition of simple impressions. Dr. Lehmann further argues that on the hypothesis of association by similarity, the observer would more frequently recognize the normal disk (when three are used) than either of the others, because it is shown more often. But on the supposition of recognition by contiguity, provided the memory picture of the normal disk be distinct, no such difference would appear, but the probability of error would be just as great with one disk as with another; while if the memory picture of the normal disk be indistinct, it would be recognized less frequently than either of the others. The results of the experiments favor this view. B, with a clear memory, making only 107 errors, misjudged the normal disk 55 times, the other disks 52 times. L, with a vague memory, making 165 errors, misjudged the normal disk 109 times, the other disks only 56 times.

In the investigation of the second case—that of recognition by means of a name or designation—Dr. Lehmann used scales with black and white as the outer parts and a varying number of intervening shades of gray. First, one of these scales was shown; then, after an interval, the different shades were shown separately, and the observer judged of their place in the scale. Here it would be expected, if recognition occurs through a name, that, when the number of shades is not greater than the names of gray in daily use, nearly all the judgments would be correct, and that when more shades are employed, many errors would occur. Experiment confirmed this supposition. With a five-part scale—black, dark gray, medium gray, light gray, white, the number of shades of gray here corresponding to the names in common use—96.7 per cent of the judgments were correct; with a six-part scale only 70.6 per cent were correct; with a nine-part scale, only 46 per cent (the minimum number of correct answers to be expected by chance would be 37 per cent with a nine-part scale). The hypothesis that recognition here occurred by means of association with a name was further corroborated by experiments with a nine-part scale, where by simply associating a number with each shade, 75 per cent of correct answers were obtained. To obtain still further proof of recognition through a name, another set of experiments were performed similar to those first described, except that before each experiment the two disks used were simultaneously shown, thus enabling the observer to note the difference between them and to give them designations. This increased the number of correct answers. In these cases, if the name is the means of recognition, then the amount of difference between the disks, provided it be sufficient to be perceived, ought to

have no influence upon the certainty of recognition. Nor should the time elapsing, nor personal conditions, nor practice have any considerable influence. The results of Dr. Lehmann's experiments gave support to all these inferences. Hence he concludes that the theory of association by contiguity explains all the phenomena of recognition, and that the theory of association by similarity, which cannot explain them all and sometimes is in conflict with experience, is superfluous. W. H. B.

The Senses of Animals. SIR JOHN LUBBOCK. pp. xxix, 292. International Scientific Series. D. Appleton & Co., N. Y., 1888.

Having been obliged to look up a great deal of literature on the subject of the senses of animals, Lubbock has put together into a book the information laboriously arrived at, for the sake of making the path of the next explorer easier than his own has been ; and he has thrown in some observations of his own, additional to those heretofore published, besides some acute criticisms of the reasoning of other observers. The result is somewhat heterogeneous, but it is interesting all the same ; it is not necessary that every book that is printed should be a harmonious whole. The list of books and papers consulted by him is very long, but Graber's latest work on the brightness-sense and the color-sense of animals seems not to have reached him at the time he wrote. This is strange, because it bears the date of 1884 ; and it is unfortunate, because it may be considered as the only absolutely thorough and scientific experimental investigation of those senses in animals that has yet been made. Graber determined the absorption spectrum of all his colored glasses and colored solutions, and the exact intensity of the light which they transmitted ; he found out how strong the preference of each animal was for brightness or for darkness before testing its preference for colors ; he offered his animals the choice between only two compartments at a time, rightly considering that to ask them to bear in mind their sensations long enough to choose between five or six was putting too great a strain upon their mental powers ; he made with each pair of colors two sets of experiments, once with one color the brighter and once with the other color the brighter. None of all these precautions were taken by Lubbock. Graber worked under many disadvantages and with much lack of means for procuring desirable apparatus ; he speaks with real grief of the fact that Lubbock, with rich laboratories at his command, did not proceed in a more systematic fashion. As regards results, concerning bees, for instance, they obtain for preference-coefficients compared with red, respectively,

	R	Y	G	B	W
Lubbock,	1	1.02	.96	1.5	1.19
Graber,	1	2	2	6	18

The only agreement is that, of the colors, blue is the favorite. Lubbock finds that white is only slightly preferred to red ; Graber that it is visited eighteen times as frequently. Graber finds besides that both blue and white with ultra-violet are three times as agreeable to bees as without ultra-violet.

The book contains much that is interesting on the instincts of some animals, especially bees, and on the intelligence which they

occasionally, though rarely, show when obstacles are put in the way of their carrying out their usual plan of action. Some wasps always put exactly five half-dead caterpillars in the cell of a male grub and ten in the cell of the female. Does this show that they can count? If not, it shows that they can do something else which answers the purpose just as well. Ants always recognize the members of their own community, though they may be 500,000 in number; this is not done by means of any sign or password, for it can still be done when the one recognized is senseless from intoxication. Experiments which were taken as showing that bees have a special sense of direction are proved to be quite inconclusive; the returning to the hive must have been done by mere recognition of known objects. There is an admirable résumé of the discussion as to whether the eyelets of compound eyes give each the whole image or only a small part of it; the latter opinion is plainly made out to be the better one. Lubbock regards it as mysterious that the rods and cones of the vertebrate eye should point outwards instead of inwards, though he says that it has some connection with development. It is difficult to see how there could have been any other arrangement, when it is remembered that the vertebrate eye is first a bladder and then a double cup pushed forward from the brain, instead of being a depression in the outer integument. The reason for this development, according to Balfour and Carrière, is simply that the portion of the ectoderm which was destined to give rise to the eyes has, in vertebrates, already been drawn in to form the brain. Neither is it mysterious that animals should see ultra-violet rays of light which to us are indistinguishable from blackness. There is good reason to believe that the reason we do not see ultra-violet is because the ultra-violet rays are strongly absorbed by the refracting media of the eye; an animal with a smaller eye would naturally not suffer so much from this inconvenience. C. L. F.

Zum Mass der Schallstärke. Dr. PAUL STARKE. Wundt's Philos. Stud. V. 1. 1888.

In completion of a former study on the question of the relation of the height of fall and of the weight of a falling body to the intensity of the sound that it produces, the author reports the verification of his former results. He finds that within the limits of error the sound is directly proportional to the height, with a constant weight, and to the weight, with a constant height. The different results of former investigators arose from their neglect of the influence of the order in time of the standard sounds and those to be compared, and of the influence of Weber's law. The sounds were produced by the fall of ivory balls of 8.07 and 16.12 grams weight, on an ebony plate from heights of from 100 to 600 mm.

Tonstärkmessung. ERNST GRIMSEHL. Wiedemann's Annalen, No. 8b, 1888. Also *in extenso* in Programmabhandlung des Realgymnasiums zu Hamburg, 1888.

Starting from an observation of Lord Rayleigh's that thin plates in a resonant column of air tend to place themselves perpendicular to the axis of the column, the author has constructed a phonometer in which the degree of rotation of a thin disk of mica measures the

intensity of the tone. The amount of rotation is measured by the deflection of a small mirror, as in a reflecting galvanometer. The tube in which the bit of mica hangs is closed at one end by a thin rubber diaphragm, and at the other by a piston, by means of which it can be adjusted to tones of different pitch. Cuts of the phonometer and curves representing the intensity of the tone of sounding pipes under different conditions are given, but the formulae for exactly connecting the amount of deflection with the intensity of the sound have not yet been reached.

Esperienze sopra i corpuscoli Vater-Pacini del mesenterio di gatto. FUBINI. Annali universali di Medic. e Chirurgia, Nov. 1887, noted in La Psichiatria, An. V, fasc. 4.

The experimenter spread the intestines and mesentery of a chloroformed cat upon a warmed glass plate, and after the animal had regained consciousness, stimulated the nerves of the Pacinian bodies. He took the dilation of the pupils as an index of painful sensation, and used for comparison those produced by the stimulation of a nerve of general sensibility. After testing with electrical, mechanical, chemical and thermal stimuli, he concludes, from the similarity of the pupil reactions in the two cases, that it is to the nerves of general sensibility that the Pacinian bodies belong. Such a relation has before been conjectured, but it cannot be held as yet demonstrated, if this experimenter has been fully reported. The responses of the pupils are too indirect and general an indication to establish the identity of the sensations in the two cases.

Influence dégénérative de l'alcool sur la descendance. A. MAIRET and COMBEMALE. Compt. Rend. CVI, p. 667, March 5, 1888.

These investigators, in prosecuting a research upon chronic intoxication in animals, have made a few very interesting preliminary experiments on the effect of alcohol on offspring. For the first experiment a vigorous and intelligent shepherd dog was given daily through a period of eight months, increasing doses of 72° absinth till he received 11 gr. per day per kilo of weight. This treatment produced hallucinations, illusions and dementia, with general paralytic troubles. When in this condition, but in a period when dosing was suspended, he was given access to a young, vigorous and intelligent female. She bore twelve pups; two were born dead, and none outlived 67 days. Three died from accident. The other seven suffered variously from epileptiform attacks, verminous enteritis, pulmonary and peritoneal tuberculosis, and besides, from lesions to be directly attributed to alcoholic degeneration—thickening of the skull, *sutures précoces*, adhesions of the dura mater to the skull, difference in weight of the two hemispheres, and fatty degeneration of the liver. The mother herself remained well. In the second experiment, a strong and intelligent spaniel bitch was given, during the last twenty-three days of gestation, from 2.75 to 5.75 grams of 72° absinth per kilo of weight. She first bore four pups, three alive and one dead, and, thirty-six hours later, two more dead. Of the three living ones, two were well formed but unintelligent; the third, a bitch, was less well developed, lazy, greedy, ungraceful in motion, short-winded, and too dull of smell to find her food in the

dark. The offspring of this degenerate creature, though sired by a vigorous and intelligent dog, show the effect of the alcohol in the third generation. No absinth at all was given, but of the three pups that she bore, one died in a few hours, was club-footed, had several atrophied toes, deviation of the apex of the heart to the right, and other physical anomalies. Another died five days old, very thin and athrepsic, with the foramen of Botal still open. And the third at fifty days of age was reported intelligent, but is touched with *carreau* and has atrophy of the hind quarters. The degeneration is in this case, therefore, greater in the third than in the second generation. E. C. S.

III.—ABNORMAL.

Apraxia and Aphasia. Dr. M. ALLEN STARR. Medical Record, Oct. 27, 1888.

The possibility of successful surgical treatment of many brain troubles has given an immense significance to all mental symptoms that can point to the seat of the lesion. It is with the practical aim of stimulating the observation and recording of such symptoms that Dr. Starr makes his exposition of apraxia, aphasia, and related states. The term "apraxia" is relatively new in neurology, and is used to cover a class of mental disturbances of which "psychic blindness" and "psychic deafness" are the best known examples. The physical basis of the concept of any object is an associated group of the residua of the sense impressions of it, retained in the various sensory centres of the brain. As the result of localized brain disease, one or more of these centres may be destroyed, or suffer a more or less complete severance of its connections with the rest. If the disease affects the visual factor, the patient may be able to see an object before him, but only know by inference from its giving utterance to a human voice that it is a human being. Or if the disease affects the auditory factor, he may be able to hear and recognize music, but not to understand words said to him. Apraxia is, in general, the "inability to recognize the use or import of an object"; and there may be as many forms of it as there are senses. Like aphasia, it is caused, so far as known, only by disease on the left hemisphere in the right-handed. In every educated person there is beside this concept-group, a word-group associated with it and made up of the residua of sensations connected with the heard, spoken, seen, and written word. By disease of the elements of this group the various aphasias, word-deafness, word-blindness, agraphia, etc., are produced ; by the severing of some of its connections, paraphasia. The author gives a brief account of these, with a schedule of the points to be examined in making a diagnosis of them ; also two tables analysing 15 cases of apraxia, and four cases from his own observation of word-deafness, word-blindness, paraphasia, etc. The article gives in brief space much matter of interest to the psychologist.

Versuch einer Darstellung unserer heutigen Kenntnisse in der Lehre von der Aphasie. ERNST MALACHOWSKI. No. 324 in Volkmann's Sammlung klinischer Vorträge.

A great point in such a presentation is clearness, and in this the author succeeds admirably. With a frequent use of schematic

diagrams he demonstrates the centres involved in speech and their connections, discusses the possible lesions of both and the resulting language symptoms, and finally makes such connections as are at present possible between the diagrams and actual brain structure. In his presentation he generally follows Wernicke.

Verhalten der musikalischen Ausdrucksbewegungen und des musikalischen Verständnisses bei Aphatischen. H. OPPENHEIM, in the Charité Annalen, XIII Jahrg. 1888; reviewed in Neurol. Centralbl. No. 18, Sept. 15, 1888.

In 16 cases of aphasia the author found 11 in which the ability to sing and to understand melodies remained, in spite of a more or less complete loss of active speech and, in most, of the understanding of spoken words. A careful analysis of the cases, however, revealed that almost every one retained the language of emotion, and to some extent mechanical automatic speech. By the presence of these the author explains the preservation of the musical capacity. The other 5 cases were not worse than some among the 11; they nevertheless showed loss of musical understanding, though two at least were known to have been able to sing. The difference of the groups leads the author to the conclusion that musical capacity may perhaps be located in a distinct area of the left hemisphere. In support, by analogy, he recalls a case observed by himself in which the memory images of numbers were destroyed by disease in the right hemisphere without disturbance of speech.

Ein Fall von Alexie mit rechtsseitiger homonymer Hemianopsie ("subcortical Alexie," Wernicke). DRS. L. BRUNS and B. STÖLTING. Neurol. Centralbl. No. 17, 1888.

The patient, a man 51 years old, had an apoplectiform attack, with disturbance of vision and right paraesthesia, but without definite paralysis. He showed a little difficulty in naming objects, occasionally was unable to do so, was a little paraphasic, and for a few days somewhat disturbed mentally. About a month after the attack he was carefully examined by the writers. His vision was found right hemianopic; he was a little awkward in the finer movements of the fingers of his right hand. There were transient signs of psychic blindness, scarcely noticeable paraphasia, and possibly slight weakening of mind. But he still had difficulty in naming objects. Occasionally he could recall their names after touching them, but sometimes had to resort to circumlocution. He could easily repeat the names when given them, or point to the object when he heard the name. He could read short words and letters at first, but later could not do so, though he could find a given letter among a few others when told to do so. He could read script letters, with a few exceptions, and, somewhat bunglingly, short written words, and could copy script. He also knew the Arabic figures, but in naming them and the script letters he was seen a number of times to make motions of writing; when his hand was put through such motions by some one else, he recognized the letters and words written, but the same failed when the forms of printed letters were followed. He could write from dictation or spontaneously, but could not after a few minutes read what he had written. In brief, the case is one of hemianopsia with almost pure alexia, the little power of reading that remained depending on the associated movements of writing.

After presenting their case, the authors discuss the kind of lesion that should correspond with such a set of symptoms. They assume for the discussion generally accepted tentative schemata, and, on the authority of Wilbrand and Wernicke, the principle that optical images, including those of words and letters, are preserved in duplicate in the right and left optical centres. It is interesting, without going further into the discussion, to remark that the preservation of the names of objects, while those of printed letters and words are lost, depends on the association of other sensations with that of sight in the case of the objects. The sight of the object calls up the associated sensations—for example, touch sensations among the the rest, or they are directly excited by handling the object. The connection between the touch centre and that of speech is uninjured, and makes possible the giving of the name. The image of the printed letter or word, on the contrary, has no other associations (or almost none), and so when the direct connection between the optical and speech centres is broken there is no byway by which the latter can be reached. Written letters and words have an advantage in associated motor sensations, and by means of them, as in the case of this patient, the spoken equivalent may be reached.

Acrophobia. Dr. ANDREA VERGA. Translated in Am. Jour. Insanity, October, 1888.

In this paper, read somewhat over a year ago at the congress of alienists at Pavia, the author makes confession of his own extreme dread of high places. Though fearless of the contagion of cholera, he has palpitations on mounting a step-ladder, finds it unpleasant to ride on the top of a coach or to look out of even a first-story window, and has never used an elevator. Merely thinking of those that have cast themselves from high places sets him tingling in the calves of his legs, his heels and the soles of his feet, or in his neck. He even experiences physical discomfort at the thought of the earth spinning through space and the imaginary possibility of the centrifugal overbalancing the centripetal force. He finds this fear growing upon him with years, as sight and hearing and the courage that they give begin to fail; even the small feats of walking in high places that were once possible to him he can no longer perform. The translator of the article also confesses the same fear. In his case the special dread that he feels on seeing a child near an open window has been given a peculiar force by the fatal fall from a window of the child of a friend. There are no doubt many other cases where the feeling has been caused or intensified by such shocking experiences.

A Rare Form of Mental Disease (Grübelsucht). CONOLLY NORMAN. Journal of Mental Science, October, 1888.

As the name of this disease signifies, the sufferer from it torments himself with endless questionings and needless investigations. The case here related was that of a married woman, thirty-two years old, who had been prepared for disease by excessive child-bearing and nursing in unfavorable circumstances. The trouble began in feelings of suffocation on waking, and the fear that if she did not rise at once the walls would fall in, she should go crazy, or something else dreadful would happen. After a time she began to feel compelled to examine any bit of straw or paper or glass that she

saw. In the street she must find out what any scrap of written or printed paper was and to what it referred. Once having passed some such bits in the evening she was unable to sleep, and finally had to waken one of her sons and go and get the papers. In doing so she shut her eyes to avoid getting into the same trouble again. For the same reason she stayed as much as possible in a darkened room. These feelings materially interfered with her housework. Thyme in the soup led to such questionings as these: "I asked myself, is that a little bit of thyme? It might be something else. That other little bit—is it thyme? I shall never be sure that all these little pieces are thyme. Can there be anything else but thyme in it? What *is* thyme?" She had to read every word in the newspaper. She was oppressed by a sense of the unreality of things; was unable to act with decision. Yielding to the impulses brought a temporary sense of relief, but denial of them led to nervous attacks, which also followed slight shocks, the necessity for prompt action, or even came uncaused. These began with a fearful sense of something to happen, of something wrong, and of helplessness, and went on to confusion, pain in the vertex, buzzing in the ears, and finally trembling and an outburst of perspiration. She was painfully conscious of her trouble and feared insanity. With cessation of nursing, etc., nourishing food and tonics, and the encouragement that she would get well, she gradually improved, was able to get control of her impulses, and finally made a good recovery.

Ueber psychische Infection. ROBERT WOLLENBERG. Archiv f. Psychiatrie, Bd. XX, H. 1.

From the study of a large number of books and articles, the author gives a comprehensive statement of present information on the subject of what has been known in France as *folie communiquée, folie simultanée, folie similaire, folie à deux, à trois*, etc., and in Germany as *inducirtes Irresein, communicirter Wahnsinn, Simultanwahnsinn*, and *psychische Contagion, Ansteckung* or *Infection*. The conditions that favor transfer of the insane ideas, the kinds transferred, the prognosis and treatment, etc., are discussed, and illustrated by brief abstracts of cases, often by many. In conclusion the author shows at length, in an interesting case of his own, how delusions of persecution grew up in the minds of two sisters, and were by degrees accepted by their father. To the article is appended a bibliography of 103 titles, of which the first 43 relate to psychic epidemics, the remainder to sporadic cases affecting only a few individuals.

Ueber Intentionspsychosen, mit Nachtrag. LUDWIG MEYER. Archiv f. Psychiatrie, Bd. XX, H. 1.

Cases in which, as a result of psychic shock, associations of such a nature are formed that the most trivial objects or events call up vast psychic disturbances, are not very rare. For this general group of cases, Meyer proposes the term "Intentionspsychosen," because in them the most striking feature, both to the patient and the physician, is morbid attention (*intentio*) to some immediately present sensation, having in mind also certain analogies to "Intentionstremor," and the dependence of the latter on intended movements. Both sensory and motor cases are included, and of them a number of illustrative cases are given—of the first, a lawyer who found himself prevented from writing in the presence of others, by attacks of dizzi-

ness, with palpitations and trembling of the hand; three ministers who became dizzy, as at a great height, when they mounted their pulpits; two agoraphobiacs, whose special difficulty was with open spaces that were paved; two locomotive engineers who were unable, on account of somewhat similar attacks connected with their business, to do their work. On the motor side the disturbances appear as obscure compulsions or inhibitions, like those experienced in high places—a merchant sees the bread-knife and his child, or later, the child alone, and has an almost uncontrollable impulse to cut its throat; a more unfortunate peasant, under similar circumstances, actually murders his child; others are urged to acts of a perverted sexual nature.

Le pazzie transitorie. S. VENTURI. Napoli, 1888. pp. xii, 94. Abstract by Sommer in Neurol. Centralbl. No. 15, 1888.

In this short treatise on temporary insanity Prof. Venturi has collected 56 cases so diagnosed. After rejecting 24 of these that had shown earlier signs of psychical irregularity, he divides the remaining 32 into six groups as follows: 1. Passionate—one incompletely observed case, corresponding to pathological anger in that it followed an insult and was followed by deep sleep and amnesia. 2. Impulsive (6 cases)—a single senseless, generally violent deed, followed at once by a short period of delirium without further violent inclinations. 3. Hallucinated (2 cases)—confused delirium following sudden hallucinations of sight. 4. Somnambulic (4 cases). 5. Melancholic (2 cases). 6. Maniacal (17 cases). Direct predisposition was found in 7 of these 17, and indirect in 11; prodromic headache and oppression were present twice. The attack lasted 6 hours in four cases; 8, 10 and 12 hours in two cases each; 3, 4, 5, 13, 14, 15 and 24 hours in a single case each. There were attempts at murder four times, at suicide twice, violence to bystanders seven times, and mere destructiveness four times. In three cases certainly there was recurrence of the attack. Deep sleep and amnesia followed in these 17 cases, though in those of the other forms the sleep was wanting eight times and the amnesia three. Prof. Lombroso furnishes a commendatory introduction, though still holding to his opinion as to the epileptic origin of temporary insanity.

On the Pathology of Delusional Insanity (Monomania). JOSEPH WIGLESWORTH. Journal of Mental Science, October, 1888.

The pathological distinction that the author suggests between mania and monomania is incisively stated as follows: "Mania begins from the top, monomania from the bottom." In mania the regulative control of the highest centres is disturbed, and the lower centres are over-active in consequence; in monomania the trouble is in the lower centres (including under that term the cortical centres primarily concerned in perception, and those below them), or still nearer the periphery. The intellect is, at least at first, untouched, but is misled by the abnormal sensory or perceptive data furnished to it from below. The constant association of hallucinations with typical delusional insanity, and the frequency with which the delusions can be traced to them, make the presumption strong that the disease that causes the hallucinations is the tap-root of the insanity. That such is the case is further made probable by certain cases of locomotor ataxy, in which the development of delusions may be followed concurrently with the advance of the disease in the peripheral nerves.

Presidential Address delivered at the Annual Meeting of the Medico-Psychological Association, Edinburgh, August 6, 1888. T. S. CLOUSTON. Journal of Mental Science, October, 1888.

The major part of this address was devoted to Secondary Dementia. General mental death is so characteristically the "goal of all insanities," that "mental disease may be defined as 'a tendency to dementia'"; and so common is it, that two-thirds of the asylum population of Great Britain are more or less demented, and two-fifths of the new cases become so. Its most typical secondary form is that which follows the insanities of adolescence. As idiocy is a failure of the brain in its period of growth, so the dementia consequent on these insanities is a failure of the highest brain tissue at the last stage of its development. The disease is not to be explained by the degenerative action of previous acute mental disease, nor by circulatory changes. It is strictly the result of bad heredity; there is a "tendency to dementia from the beginning." To know what this goal of insanities really is, and to prevent their reaching it, is the problem of psychiatry.

The address was eminently successful in calling out discussion (*vide* the report of the proceedings in the same number of the *Journal of Mental Science*). It was discussed by Drs. Tuke, Savage, Wiglesworth, Ireland, and others, almost every speaker taking issue with Dr. Clouston on one point or more.

IV.—MISCELLANEOUS.

Memory and its Doctors. DR. E. C. PICK. London, 1888.—*Memory. What it is and how to improve it.* DAVID KAY. London, 1888. "*Loisette*" *exposed, together with Loisette's Complete System of Physiological Memory.* G. S. FELLOWS. New York, 1888.—*Memory Systems, Old and New.* A. E. MIDDLETON. With a bibliography and other matter, by G. S. FELLOWS. New York, 1888.

The appearance of these volumes testifies more than to anything else to the great popular interest in psychological matters, especially when any practical advantages are to result; for the idea has not yet been entirely abandoned that some royal road to knowledge is still to be found, some mysterious method of which a favored mortal possesses the key still to be revealed. While psychology is supposed to hold some definite position regarding such themes, hardly any confidence is placed upon these opinions in a matter of practical application. Thus the professional memory-teacher gains success from a public that ought to know better.

Dr. Pick's little volume presents quite modest claims. He bases his system upon natural acquisition by real labor, not hampering the pupil by associations artificially imposed, but simply advocating the good effects of method and an attention to one's associations. Especially do the sound portions of his treatise become prominent when we contrast them with the shallow attempts of his predecessors, of whom he gives a concise and convenient account. There we read of associations of dates with the rooms of a house, with harsh sound combinations and the places in a magic square. Even a memory pill and a memory diet was advocated. Prominent examples of mnemonic feats are also entertainingly given. This primer can be recommended as a pleasant introduction to the topic,

and is especially interesting by the success of Dr. Pick's teaching, and the further fact that it is from him that Loisette has borrowed so largely without acknowledgment.

Mr. Kay's more comprehensive work will also find a large public. He approaches the problem from a broad psychological point of view, with no haste to reach astounding practical results, and a sound interest in the educational value of psychological principles. He begins with the physiological concomitants of memory, devoting a chapter to the relation of body and mind, and others to the description of the senses and their functions, the nature of mental images, the rôle of the unconscious, and the like. The dependence of a sound memory upon close attention furthered by a living interest, upon active repetition, upon intimate association with centres of interest, are all well described, with a wealth of references to general psychological literature. It is a pity that his physiology is at times not strictly accurate, and still more so that he seems to be unacquainted with the recent German contributions to the topic.

The last two volumes are of greater popular than scientific interest. They are called out by the ridiculous pretensions and peculiar methods of "Loisette." The pledge to secrecy which he imposes is here disregarded and the entire lesson papers printed in full. This may have the good effect of showing the folly of trusting one's mental culture to the guidance of so artificial a system. The exposure led to the withdrawal of the book from the market by legal procedure, and to the publication of the last work on the list, in which the account of the proceedings is given. Mr. Middleton's account of memory systems is convenient but superficial. Great credit is due Mr. Fellows for his useful bibliography of the subject.

J. J.

Memory. Its Logical Relations and Cultivation. F. W. Eldridge-Green. London, 1888.

In opposition to most physiologists, the author endeavors to prove that memory is "a definite faculty," having "its seat in the basal ganglion of the brain." Rejecting phrenology, the author nevertheless comes dangerously near the position of the phrenologists, by dividing the mind into a great number of faculties. The "faculty" of memory he divides into "sensory memory" and "motor memory," and locates the former in the optic thalami, the latter in the corpora striata. Rules are given for the cultivation of both forms of memory. Those relating to the motor memory have to do with the learning of co-ordinated muscular movements.

Il fenomeno della ricordanza illusoria. Francesco Bonatelli. Rendiconti della Reale Accademia dei Lincei. Vol. IV, fasc. 4, February 19, 1888.

As an example of an illusion of memory, the author relates a dream which he seems to have had more than once. He dreamed of occupying a certain set of apartments, and each time remembered having lived in them years before; they were, however, on waking reflection, entirely different from any he had lived in. That this was not a case of recollection from dream to dream he believes, because with this exception his dreams have no similarity one with another, and because, in the waking state also, one is sometimes

convinced that circumstances in which he has certainly never been before, are a repetition of others experienced in the past. In explanation of both phenomena he suggests that this conviction arises from an obscure emotional accompaniment of the perception. In peculiarly excitable states of the nervous system (as in vivid dreams, or when one is in strange places), parts of actual perceptions, as is normally the case, pass out of the focus of consciousness, and returning an instant later, meet changed conditions into which they do not fit, and therefore appear to be recollections. This rapid passing out of and into the focus of consciousness (or the physical concomitant of it) is not perceived, if we conceive the author rightly, but gives rise to the emotional accompaniment just mentioned.

Association by Contrast. M. PAULHAN. Revue Scientifique, Sept. 1.

The general law, applicable as well to higher states of consciousness as to sensations, is formulated, claiming that every psychic state tends to be accompanied (simultaneous contrast) or followed (successive contrast) by an opposite state. In sensation, the phenomena of complementary colors, of warmth following a sensation of cold, are typical. In motion, every contraction of a muscle involves the contraction of the antagonistic muscle. When moving and suddenly stopped, we seem to be going in the opposite direction. In the sphere of judgment, alternatives are ever present, an argument *pro* calls up another *con*. A vacillating temperament is characteristic of some types, while in the hypnotic subject it is strikingly absent. Morbid instances arise in which every idea realises its opposite, with alarming results. Again, depression follows joviality, and even the alleged phenomena of "psychic polarisation" would come under this law. Examples from all phases of psychic activity are brought together to show the wide bearings of the law of contrast. J. J.

The Geographical Distribution of British Intellect. Dr. A. CONAN DOYLE. Nineteenth Century, August, 1888.

Following the line of investigation inaugurated by Mr. Galton, Dr. Doyle examines the relative fertility of distinctive portions of the British Kingdom with reference to the production of celebrities. The degree of eminence recognized by Dr. Doyle is lower than that usually treated in such researches, and includes such as would deserve mention in a standard biographical dictionary like "*Men of the Time*" and yet rank higher than local celebrities. He selects about 1150 such men eminent in literature, art, music, medicine, sculpture, engineering, law, etc. These are found to contain 824 English born, 157 Scottish and 121 Irish, while 49 were born abroad. England would thus have one celebrity to 31,000 of population, Scotland one to 22,000, and Ireland one to 49,000. Wales, if counted separately, would have one to 58,000. London produces much more than its share of eminence, claiming 235 of the 824 Englishmen, or one to every 16,000 of the population. Dublin shows still better with 45 celebrities, one to 8500, and Edinburgh leads easily with 46, or one in 5500. While the chief cities are thus the intellectual centres, Dr. Doyle thinks the very greatest intellects come from the country. London is especially strong in artists and men of science. The standing of the various counties is detailed, making the eastern and southern counties superior to the northern and midland, "while

that portion of Scotland which lies between the Forth and Clyde on the north, and the English Border, is in the proud position of having reared a larger number of famous men in the later Victorian era than any other stretch of country of equal size." Other conclusions are "that agricultural districts are usually richer in great men than manufacturing or mining parts." And that, "if a line be drawn through the centre of Lincolnshire, it will be found that the poetry of the nation is to the southern side of that division"; it being regarded that, with a few notable exceptions, music, poetry, and art reach their highest development in the south, while theology, science, and engineering predominate in higher latitudes. J. J.

Facts and Opinions relating to the Deaf, from America. ALEXANDER GRAHAM BELL. London. 8vo, pp. 196.

This report to the Royal Commission of the British Government to inquire into the condition of the deaf, is of great value to students of this interesting class of defectives. Five questions are treated, mainly by the statistical method and the collation of the opinions of experts. 1. Visible speech. The fact that of 31 institutions in which it has been introduced it has been continued in only 17, argues against its universal applicability. 2. The development of latent powers of hearing in the partially deaf is ably discussed, with the result that the future holds out bright prospects in this direction. 3. The most important topic is that of the heredity of the deaf-mute as a class. Here the experience of superintendents of asylums goes to reducing the evil effects of intermarriage, some holding that the additional happiness thus brought about is more than a compensation for the slightly increased chances of a deaf offspring; others holding that consanguinity is a more potent factor than deaf-mutism, while still others make a difference between the congenitally deaf and those who become so later in life. The scientists, on the other hand, are unanimous in their agreement with Prof. Bell's position that the marriage of the deaf-mute with the deaf-mute is an ever increasing factor in the production of deaf-mutism, and that, if continued, it must end in establishing a deaf-mute variety of the human species. 4 and 5. Under these heads various usages and modes of instruction of different schools are summarized. J. J.

A Method of Examining Children in Schools as to their Development and Brain Condition. FRANCIS WARNER. Brit. Med. Jour., Sept. 22, 1888.

In the rapid observation of children in these particulars, very much can be learned by attention to two classes of facts: "(*a*) the form, proportions, and texture of the visible parts of the body; and (*b*) the signs of action of the central nerve-system, as seen in the muscles producing movements or attitudes or balances of nerve-muscular action." The first shows the development and nutrition; and in the condition of the special features often lie indications of mental weakness. The second shows, in variations from the normal, nerve-muscle weakness, fatigue, and excitability. Besides such things as these and starvation, the doctor has found hare-lip, congenital cyanosis, rickets of the skull, brain disease with congenital syphilis, all grades of idiots, and, with the help of the teacher, *petit mal.* In the Day Industrial School, of Liverpool, 14 per cent of the

281 children showed defective development or nervous symptoms, but there was little exhaustion. Of the 106 children in one school for truants, 40 per cent were defective ; of the 47 in another school, 6 boys of eight pointed out as "specially bad or troublesome" seemed to have some physical basis for it, and ten "good, quiet and decent" boys showed signs of nerve weakness.

Eye-mindedness and Ear-mindedness. JOSEPH JASTROW, Ph. D. Pop. Science Monthly, Sept. 1888.

This paper is an interesting résumé of present information, increased from the author's own observation, on the effect on the mental complexion of a predominance of sight, hearing, or, more briefly, of touch. An eye-minded man learns easiest and does his best work with his eyes, an ear-minded man with his ears. The importance of regarding these differences in the conduct of mental life, either one's own or that of pupils, sets in a practical light the methods of determining the dominant sense, to which Prof. Jastrow devotes considerable space.

Genius and Precocity. JOSEPH JASTROW, Ph. D. Journal of Education (London), July 1, 1888.

This paper follows the lines of that of Sully (*Nineteenth Century*, June, 1886) ; but by stricter definitions of precocity and greatness, the author is able to go a step beyond the connection there demonstrated between precocity and eminence. He shows that among men of transcendent greatness (men of action and statesmen are excluded in both papers), the proportion of the precocious is nearly twice as great as among the merely eminent. An examination of the biographies of the specially precocious shows that while they produce work earlier than other great men (at about 15½ years on the average), they do not do great work earlier (29½ years), and do their greatest work, if anything, later (46½ years as against 44½), and that they do not die earlier. On the other hand, if the list is made still more exclusive, reduced to veritable *Wunderkinder* (the numerical basis being now, of course, small), the age of producing work of these degrees of excellence is decreased, for the great work and the greatest work, by about four years, and for the age at death by as many as six.

Insomnia and Other Disorders of Sleep. HENRY M. LYMAN. pp. 229. W. T. Keener, Chicago, 1885.

To the present generation, books on sleep and its disorders should be specially welcome. This one opens with a discussion of the nature of sleep, giving an elementary statement of the theories of Obersteiner and Pflüger, then passes to insomnia and its treatment, and adds something on dreams, somnambulism, and hypnotism. The discussion is not very thorough, the most useful portion being the first ninety pages. Beyond this, the anecdotal method of presenting the facts is adopted, and while this is usually entertaining, it is not always very valuable. Perhaps our information regarding sleep has not yet fairly passed from the descriptive phase with which the science of any subject begins, but there is certainly enough experimental work to be discussed to furnish the basis for a somewhat less poetical and more scientific book than the one in question.

Aerztliche Philosophie. Dr. G. E. RINDFLEISCH. Festrede zur Feier des dreihundert und sechsten Stiftungstages der Königl. Julius-Maximilians-Universität, gehalten am 2ten Januar, 1888. Würzburg: Commissions-Verlag von Georg Hertz. 1888. 20 pp. 4to.

This address by the Ordinary Professor of Pathological Anatomy, Pathology and History of Medicine at Würzburg is intended to express to young physicians opinions concerning the medico-philosophical questions of the present. The author declares his pity for those "who, with the necessary recognition of mechanism in all natural processes, have lost the spirit of a non-materialistic view of nature," and excludes himself from their number. Though we may measure the phenomena presented in time and space, still "on all that is there is the mark of infinity." And this "infinity" is not compassed by the circle of our measurements and determinations. Yet all straining after the unknowable on the part of the medical investigator is to be lamented. "Earnest, upright and conscious reserve toward the eternally undiscoverable, and indefatigable labor in the investigation and use of what is accessible to our understanding," according to the author, expresses the true scientific spirit. It is disregard of this spirit, he thinks, which has brought discredit on the history of medicine. A crass materialism and a superstitious spiritualism are both one-sided products of human weakness. "Neo-Vitalism" is selected as the theme for the positivistic text given above. The history of the Cell-Doctrine is briefly sketched and its present condition outlined. The conclusion is that no decisive philosophical datum has resulted from this line of investigation. Aside from its word of caution to students of biology, and its emphasis of reserve toward ultimate questions, and of loyalty to exact methods, the address has no special significance.

D. J. HILL.

Einleitung in die Psychologie nach kritischer Methode. PAUL NATORP. pp. 129. Freiburg, 1888, J. C. B. Mohr.

The professed object of this brochure is to state, not to solve, the problem of psychology. It is written from the purely introspective and speculative standpoint, with an entire unconsciousness of what has been done by Wundt and his school to bring the subject within the reach of experiment. There is a brief section discussing the question whether psychology can be made a natural science or not, but even in this there is no approach to a recognition of experiment. The work has no interest outside the technical investigation of transcendentalism.

NOTES.

Moderate illumination of the peripheral parts of the retina by light concentrated with a lens on the sclerotic increases the acuteness of vision in most normal eyes. Letters are made to appear blacker, and some not before legible become so. Only when the illumination is considerable does it interfere with vision. In a number of eye-diseases, however, the least side illumination produces disturbance at once.—(SCHMIDT-RIMPLER, *Ber. d. XIX Vers. d. ophth. Gesellsch. zu Hamburg.*)

Charpentier gives, in the *C. r. Soc. de Biologie*, Mai 19, 1888, the results of experiments showing that the difference in brightness of two neighboring illuminated surfaces can best be seen with the central parts of the retina, except a less sensitive central spot. The discriminative sensibility falls off rapidly toward the periphery. It is better, however, on the outside than on the inside of the field of view, and above than below.

The same experimenter (*ibid.* Mai 16, 1888), taking neighboring surfaces, one illuminated continuously, the other instantaneously, adjusted the sources of light so that the surfaces appeared equally bright, and then weakened both illuminations equally. The continuously illuminated surface appeared the brightest, and indeed when the illumination was very much reduced was the only one seen. When both surfaces were illuminated for different lengths of time and yet made to appear equally bright, and the size of their images was then reduced by a concave lens or by withdrawing from them, the one most briefly illuminated appeared the clearer and brighter.

The number of the visual units in parts of the retina has been redetermined by Wertheim after the method of C. du Bois-Reymond (*v. Graefe's Arch. f. Ophth.* XXXIII, 2). He made use of a plate of tinfoil pierced with 460 holes of 0.24 mm. in diameter. The distances were found at which the holes seemed to form lines and the lines a surface. Calculations from the first give 74, from the second 147 visual units in the centre of the fovea, to a square 0.01 mm. on the side. Going out from the middle of the fovea toward the temple, the following results were found for the same area:

Distances in mm.	0.	0.15	0.3	0.45	0.6	0.7	1.0	1.55	2.4
From first measurement, .	74	37	27	28	24	18	15	13	10
" second " .	147	77	53	53	50	40	33	27	20

The sudden fall in the number between 0.6 and 1.0 mm. is probably due to passing into the region of more numerous rods outside the yellow spot.

The time required to perceive the form of images that do not extend beyond the yellow spot has been found by Nordmann (abstract in *Jahresb. der Anat. u. Physiol.* Bd. XVI, Abth. 2, p. 176) to be for the adapted eye inversely proportional to the illumination. With constant illumination the time increases inversely as the size of the image. This seems true also of the unadapted eye, but the absolute time is longer. The illumination used in the experiments was furnished by a candle behind ground glass, the time was regulated by a pendulum, the objects were Snellen figures (*Haken*).

It is familiar that faded after-images may be recalled by rubbing the eyes. Dr. Ch. Féré finds (*C. r. de la Soc. de Biol.* July 30, 1887) that the same can be done in other ways, as for example by applying a tuning-fork to the skull. He finds also this somewhat similar phenomenon (*ibid.* Dec. 10, 1887). A sensation too weak to reach consciousness alone may succeed in doing so with the help of a strong one in some other sense. An hysteric looks at letters that are too far away to be read. They are then covered, a strong sensory stimulus administered, and the letters are read from their previous impression. It would be extremely interesting to know whether such experiments are possible with normal persons.

In the opinion of Bjelow (*Westnik. Opht.* IV, 3 and 4; Abstract in *Jahresb. Anat. u. Physiol.* Bd. XVI, Abth. 2), the rest position of the eyes is divergent. The eye, however, does not necessarily return to it when simply covered. Even the introduction of prisms cannot cause the eye to diverge beyond its rest position.

A. Chauveau, after a study of the mechanism of iris movements (*C. r. Soc. Biologie*, April 14, 1888), pronounces for the non-existence of a dilator muscle of the iris, that office being performed by simple elastic fibres. He bases his statement chiefly on the fact that the latency period for both contraction and dilation is the same, about half a second, which could hardly be the case if a *dilator iridis* were really present and innervated through the roundabout way of the cervical cord and cervical sympathetic. In this view he has the support of Rouget, Grühagen, Retterer, and others. The latency period was determined by observation of the diffraction circles of a set of needle-pricks in a card held before one eye while the other was darkened and lighted.

Eye-strain is a recognized factor in the production of many nervous troubles. Dr. Geo. M. Gould points out (*Med. and Surg. Reporter*, Sept. 29, 1888), in addition, an obvious mental and educational effect. The child for whom the process of learning is made laborious or painful by imperfect vision, is so far handicapped, and as a result may appear stupid and actually be disinclined to study. In one of three cases given in illustration, correction of the optical defects seems to have been followed by most happy intellectual results.

A convenient chart for rapidly testing the vision of school children and others is that devised after the principle of Snellen by Prof. Hermann Cohn. The essential part of the chart is a set of thirty-six capital letter E's, arranged in a square, six on a side. Each

letter is itself about a third of an inch square. They face up, down, right and left. Any one that can tell their facing at the rate of 1 per second with proper illumination, at a distance of six metres, has the acuteness of vision assumed by Snellen as unity, $\frac{6}{6}$. If one has to come a metre nearer to do so his vision is $\frac{5}{6}$, and so with the rest. The chart is provided with four brass eyes, one at the middle of each side, so that it can be given four positions, and the possibilities of learning the positions by heart made less. A set of lenses in wooden frames for ready handling would also allow the fixing of the degree of short or over-sightedness. When the sight of one pupil has been established as normal, the chart may be used to find whether or not the illumination of the school-room is sufficient, by finding whether he can tell the facings at the normal distance.

In *du Bois-Reymond's Archiv*, No. 3–4, 1888, L. Jakobson proposes the use of the receiving telephone thrown into motion by changes in intensity of a varying current, for the numerical determination of the acuteness of hearing. It may be assumed on physical principles that the variation in the intensity of the sound is proportional to the square of the variations in the current. Gradations of the current are to be introduced by the insertion of shunts of different resistance. While these resistances remain smaller than the rest of the circuit, the variation of the current in the telephone will be, with an error of not more than 2.5 per cent, directly proportional to the resistances used. It is thus easy to so adjust the sound as to find the auditory threshold. The author suggests an apparatus in which a tuning-fork vibrating before the poles of an electro-magnet is used to produce with regularity the original current.

Hernicke (*Untersuchungen über den Temperatursinn*, Inaug. Diss. Bonn, 1887) finds that in tabes dorsalis the discrimination for differences of temperature is much below the normal.

In a case reported by Dr. J. H. Lloyd of Philadelphia, to the last meeting of the Am. Neurol. Assoc. (*Boston Med. and Surg. Jour.* October 11, 1888), it was found, after the arm area of the cortex had been removed for Jacksonian convulsions, that though the perception of form and motion was abolished, the pure sensation of touch was not in the least disturbed.

The self-adjustments of plants with reference to light and gravity have already been the subject of careful study. In the *Sitzungsbr. der Phys.-Medic. Gesellsch. zu Würzburg*, Jahrg. 1888, No. 1, J. Loeb makes a preliminary communication of experiments of his that go toward demonstrating the same in animals. Bilateral animals tend to place themselves so that the rays of light fall parallel to the median plane of their bodies. There is, however, this difference of habit: some turn their oral pole and ventral surface to the light, others their aboral pole and dorsal surface, and in exceptional cases the aboral pole and ventral surface. With equal intensities of illumination, animals seem far more affected by the strongly refracted rays, but the influence of the less refracted rays increases with increased differentiation of organs; so that, for example, flies of

musca vomitoria were clearly affected by red light, while the eyeless grubs, though under other circumstances heliotropic, were not. The animals seem to adjust themselves rather to the direction in which the rays pierce their tissues, than to light and shade. The intensity of the light also is important, for only within certain limits of intensity are the phenomena to be observed, and the quickness and precision of the adjustments are different for different intensities. That the reaction is really to light and not to heat, is proved by the fact that diffused daylight, light passed through concentrated alum solution, and the more refracted rays of the spectrum, all produce the turning, the last in a greater degree than the hotter rays. In the author's opinion these phenomena are related to "seeing," and indicate that what we are wont to call a psychic function is in some sense a property of all living matter.

In response to gravity, flies with clipped wings set themselves, other things being equal, so that their median plane is vertical and their head is up. In other experiments, roaches very strongly objected to having gravity act perpendicularly upon their ventral surface.

In somewhat the same direction, but upon lower organisms, were the experiments of R. Aderhold (*Jena'sche Zeitschr. f. Naturwissenschaft*, XXII, 1–2, 1888). He found that Euglena and others are not influenced in their direction of movement by water currents, *i. e.* are not rheotropic; they are, however, aërotropic and geotaxic. The last two characteristics would be of use to Euglena in coming to the surface when accidentally covered up in any way. Other organisms, including diatomes and oscillaria, seem to regard none of these things and respond to light alone. Desmids are positively phototaxic with weak light and negatively with strong light. Algae set themselves with a definite position of their axis toward the light because they tend to move toward it. The angle at which they place themselves with reference to the surface below them in moving forward seems always to be that at which friction is least.

Two peculiar cases of aphasia have been observed by Dor (*Rev. général d'Opht.* 1887, p. 155) in which, while color perception remained, the color names were lost.

In a study of the mental state of the hereditarily degenerate who do not reach full insanity (*Arch. génér. de méd.* March–April, 1888), Ballet relates, with other cases, one of a bookbinder, of convergent bad heredity, 37 years old, and a polymath in a small way. After a long period of daily attacks of palpitation, shortness of breath, aura-sensations, etc., he experienced while reading an attack of a severer sort, with loss of consciousness. After one repetition this gave place to agoraphobia. For fear of other attacks he stopped reading, and even the sight of a book or an unfamiliar word threw him into mental anguish, palpitations, and perspiration. Still later, on reading a word from a placard he felt suddenly uneasy, had to repeat the word several times, but for all that, forgot it except one syllable ; then that seemed to rise up to his brain and he fell down unconscious. Not till the next day did he recall what went before the fit. After a second such attack he attempted suicide, and developed slight delu-

sions; and after a third, became paralysed in the left arm and leg. He had a contracture on the right side of the face, complete anaesthesia on the left, and a hysterogenic point in the lumbar region, and showed transfer phenomena with a magnet. His memory and intelligence suffered, and he was sensitive and irritable. He showed a misshapen skull, sexual impotence, *folie de doute* and arithmomania. Attempts at reading ended in unconsciousness, or he stopped at some word and declared that his tongue forbade him to say it; when at other times he read past it, he saw it advance along the line and his field of view became dark. The loss of consciousness after the "onomatomaniac" seizures, Ballet attributes to an hysterical attack induced by them, and not to the seizures themselves.

Dr. Frigerio reports a case of persistent refusal of food (*Archiv italian. per le malat. nervos. ecc.* 1888, XXV, p. 98), on the part of a syphilitic paranoiac. Though willing to make repeated trials, he saw his food each time magnified to such an extraordinary heap that he was turned back. Anti-syphilitic treatment produced improvement in other respects but not in this, nor was the illusion destroyed by excluding accommodation with atropine. Ophthalmoscopic examination only showed signs of syphilitic retinitis.

In a paper presented before the psychological section of the British Med. Assoc. (in abstract in *Brit. Med. Journal*, September 1, 1888, p. 484), Dr. Oscar T. Woods gave this interesting case of *folie à quatre*. Four members of a family, mother, son, and two daughters, were infected by a third older daughter, and to that degree that a child of the family was murdered by the mother. They were found violent and partly naked, with the delusion "that having killed the fairy, they were freed from their sins and went to heaven." The four recovered in a couple of weeks; the daughter by whom they were infected remained insane.

Nervous children are frequently on the verge of being overpowered by their own imaginations, and offer a ready soil for the growth of hallucinations. Moreau, in an article on the hallucinations of children (*L'Encéphale*, No. 2, 1888), collects ten cases, mostly from recent literature, of children between the ages of two and fifteen thus affected. The exciting causes fall into two classes: moral causes—for example, horrible stories, excessive religious training, life with others subject to such disorders, the terrors of the night, over-vivid impressions of some kinds; and physical causes, like fevers, poison, and disorders of digestion. Hallucinations of sight and hearing are by far the most common, and they are almost always sad and terrifying—devils, monsters, spirits, witches, threatening men, and the like.

Under the title of "*Ein genesener Paralytiker*," Schaefer gives, in his Inaugural Dissertation (Berlin, 1887), some references to the literature of recoveries from general paresis, describing in detail the case of a man who recovered under his care. The recovery is, in all cases, consequent on severe suppuration.

The *Journal of Mental Science* for October 1887 and April and July 1888, contains a long and interesting review, from the standpoint of the alienist, of Dowden's Life of Percy Bysshe Shelley. The poet is shown wavering along in the region between sanity and insanity, sometimes upon one side, sometimes on the other. He was of bad nervous heredity, undersized in brain, unstable, very susceptible to the other sex, had hallucinations and delusions, and yet withal a genius of a type that critics have not scrupled to call angelic.

A stunted brain is not a guarantee against disease of such mind as accompanies it. Dr. Henry M. Hurd (*Am. Jour. of Insanity*, Oct. 1888) shows, with illustrative cases, that insanity is to be found among imbeciles above the lowest grade. The latter are indeed only irritable and impulsive, but those a little higher are perverted, and their impulsive acts are those of insanity and may reach suicide and killing. They do not, however, have delusions and their mental disturbances are transient. In the highest class there are manias and melancholias, simple or with delusions, and their course is not different from that in other insane. Pianetta reported to the congress of Italian physicians at Pavia (*Riv. speriment. di Freniatria*, XIII, 1888) observations on 114 imbeciles in the asylum at Imola. 51 were demented and quiet, 44 demented but occasionally excited and violent, and 19 showed clear mental disease on a foundation of imbecility. Of these 19, ten were maniacal, eight were melancholiacs, and one a paranoiac. The degree of imbecility in these cases was slight, but the road is short to worse conditions.

For the purpose of comparison with similar observations made by others on the disturbances of reading in cases of progressive paralysis, F. Kraemer has studied the loud reading of the uneducated, the aged, and the non-paralytic insane (*Verhandl. d. Physik.-Med. Gesellschaft zu Würzburg*, XXII, 4.) The errors that he found were for the most part insignificant, except for a few of the aged, chiefly those in whom senile dementia had already begun. They, like the paralytics, distorted many words, and added others that had no similarity in sound or sense to those in the text. The non-paralytic insane, even where their alienation was extreme and of long standing, read for the most part with as few errors as the sane.

In his oration on "Education in its relation to Insanity," before the alumni of Haverford College, 1887, Dr. Robert H. Chase furnishes thought for both optimists and pessimists in educational matters. He emphasizes the fact, already known, that on the one hand, improper and unhygienic education is a fruitful source of insanity, and on the other, that the bulk of the insane come from among the uneducated, and that proper education is one of the surest safeguards against its increase.

Cionini (*vide* report of Italian medical congress at Pavia, *Riv. speriment. di Freniatria*, XIII, 1888) measured the thickness of the cortex in 15 cases of paralysis, making 150 measurements on each, 50 in the portion forward of the rolandic region, 50 in that region, and 50 in the portion behind it. He found a general thinning of the whole cortex, most marked in the posterior central convolution, less so in

the anterior, still less in the parts further forward, and least in the rearward parts. It is less marked in the left hemisphere than in the right. Other things being equal, it is thinner in the depths of the sulci, and on the basal and median surfaces, than on the convolutions and convex surface.

A. Borgherini reports, in the *Riv. speriment. di Freniatria*, XIII, 4, the examination of the brain of a dog that had shown clear symptoms of ataxy. The cerebellum was only slightly small, but showed microscopically extended atrophy, irregularly scattered over its cortex, and especially marked in the worm.

Exner and Paneth (*Pflüger's Archiv*, Vol. 40), in the course of certain other experiments, extirpated the gyrus sigmoideus on one side in six dogs. In five of them, disturbances of sight took place which lasted for about four weeks. In six other cases, where the injury was of slightly greater extent, the same "crossed sight disturbances" were occasioned—injury on the right side caused nearly total blindness in the left eye, on the left side in the right eye. Recovery took place in from 7 to 37 days.

A somewhat full discussion of Brown-Sequard's hemilateral lesion of the spinal cord is given by Nolte (*Brown-Sequard'sche Halbseitenläsion des Rückenmarks*, Inaug. Diss., Bonn, 1887). The body of the dissertation is taken up with the description of a man in whom a knife-wound between the second and third dorsal vertebrae caused a hemisection of the cord and a clear exhibition of the corresponding symptoms. The author does not contribute at all to the anatomy of the subject, but the clinical literature is largely given.

Frigerio endorses Ferrier's location of the centre for smell in the *cornu ammonis* (*vide* report of Italian medical congress at Pavia, *Riv. speriment. di Freniatria*, XIII, 1888), on the ground of a case of traumatic paranoia, with hallucinations of smell, in addition to tactual and auditory illusions. The autopsy showed extreme atrophy of that region on the left side.

The ideal way in which to instruct students in the gross anatomy of the central nervous system, is by means of fresh material and elbow advice, both in unlimited quantities; but when students are counted by the hundreds, and the time is very limited, this plan must be modified for the good of the greatest number. No course is better, perhaps, under these conditions, than modelling the parts of the brain on an heroic scale before the class. Prof. John Curtis, of the College of Physicians and Surgeons of New York, has made extensive use of this means, and his models in clay, photographs of which have been made by Dr. Warren P. Lombard, show how admirably some of the difficult points in brain anatomy can be thus elucidated.

W. P. Herringham, M. B., gives in *Brain*, July, 1888, an interesting note on heredity, under the title of "Muscular Atrophy of the Peroneal Type Affecting many Members of a Family." Members of

five generations are listed. The strange feature of the inheritance, and one that the author says is peculiar to the family and not the disease, is the frequency with which perfectly healthy daughters of the line have transmitted their fathers' infirmity to their sons but not their daughters. No daughter of the line is reported as diseased.

The following method of studying cerebral localization in a way different from those of Hitzig and Ferrier, is privately suggested by Dr. Clevenger. He says: "If peripheral points all over the body be stimulated electrically, successively, and the bared brain of the animal, either anæsthetized or recently killed, be covered with paper saturated with the ferro-cyanide of potassium liquid, such as was used in Bain's telegraphic recorder, through which paper the circuit may be completed by a checker-board arrangement of many small metallic plates, it seems probable that the least resistance channels will be indicated by the blue discoloration on the paper at points corresponding very likely with the centres for the stimulated peripheral points. Galvanometric deflections could also be observed. Diffusion would surely be prevented by thus affording many separate points of escape for the current. As to whether it would travel in the course of nerve strands remains to be seen, but with proper precautions I cannot see why it should not do so."